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PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From December 5, 1889, *to April* 24, 1890.

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PROCEEDINGS

OF

THE ROYAL SOCIETY.

December 5, 1889.

Sir G. GABRIEL STOKES, Bart., President, in the Chair.

The President announced that he had appointed as Vice-Presidents—

The Treasurer.
Professor Alfred Newton.
Sir Henry E. Roscoe.
Professor A. W. Williamson.

Professor T. McKenny Hughes was admitted into the Society.

Pursuant to notice, Professors Stanislao Cannizzaro, Auguste Chauveau, and Henry A. Rowland were balloted for and elected Foreign Members of the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Remarks on Mr. A. W. Ward's Paper 'On the Magnetic Rotation of the Plane of Polarisation of Light in doubly refracting Bodies.'" By O. WIENER and W. WEDDING, Physikalisches Institut, Strassburg i. E. Communicated by LORD RAYLEIGH, Sec. R.S. Received October 24, 1889.

In the above-mentioned paper Mr. Ward* communicates theoretical and experimental investigations which, as far as they are correct, are in their essential parts already published, and indeed somewhat more completely in three papers by Gouy† and ourselves,‡ the latter inves-

* Ward, 'Proceedings of the Royal Society,' 1889, vol. 46, p. 65.

† Gouy, 'Journal de Physique,' vol. 4, 1885, p. 149, "Sur les Effets simultanés du Pouvoir rotatoire et de la double Réfraction."

‡ Wiener, 'Wiedemann's Annalen,' vol. 35, 1888, p. 1, "Gemeinsame Wirkung von Circularpolarisation und Doppelbrechung, geometrisch dargestellt."

Wedding, *ibid.*, p. 25, "Die magnetische Drehung der Polarisationsebene bei wachsender Doppelbrechung in dilatirtem Glas."

tigations having been suggested by Professor Kundt. Experiments on the rotation of the plane of polarisation of light by means of interrupted currents, like those of Bichat and Blondlot, are here excluded. Since the above-mentioned papers have not been noticed by Mr. Ward, and are hence probably not well known, it may be of interest to reproduce here their essential contents.

Gouy shows that in a body, which at the same time has the power of double refraction and of rotation, certain vibrations, to which he gives the name "privilégiées," are propagated unaltered. These vibrations play exactly the same part in such bodies as the linear components in, and normal to, the principal section in the case of ordinary doubly refracting bodies, and as Fresnel's two circular components in the case of ordinary rotating bodies. In bodies which at the same time have the power of double refraction and of rotation, these two privileged components of vibration take place in opposite directions in two ellipses, whose major axes are one in the plane of principal section and the other perpendicular to it. The ratio of the axes and the difference of phase of the components are calculated by Gouy from the constants of double refraction and rotation.

For the complete solution of the problem of the propagation of light in such a body we only require to resolve any incident vibration into its two privileged components, and to compound them to a single resultant on emergence. The solution of this problem is contained in the above paper by Wiener, which was intended to supply the theoretical foundation for Wedding's investigation. It was especially important to determine how the rotation, due to the rotational power of the body, is disturbed by double refraction. It was found that the rotation alters periodically with the thickness of the plate, and with strong double refraction may even become negative, that is to say, in the opposite direction to that due to the rotational power. The rotation is zero in the neighbourhood of those places where the difference of phase of the linear components, due to double refraction, is a multiple of π, and not, as Mr. Ward thinks, of $\pi/2$. The general result of Wiener's paper may be quoted as follows:—

"Herrscht bei der gleichzeitigen Wirkung von Circularpolarisation* und Doppelbrechung die eine vor, so wird die andere theilweise verdeckt. Eine starke Doppelbrechung drückt die Drehung der Circularpolarisation und eine starke Circularpolarisation das Elliptischmachen der Doppelbrechung herab."

Wedding's investigation is of purely experimental nature, and consists of two parts. The first is occupied with magnetic rotation in stressed glass, and completely confirms by experiment the con-

* By "Circularpolarisation" is understood what we have here called rotational power.

clusions of the theory, especially with regard to zero and negative rotation. It concludes with the words:

"Ebenso darf man bei Krystallen, an denen man keine elektromagnetische Drehung beobachten kann, nicht den Schluss ziehen, dass ihnen keine Verdet'sche Constante zukäme, weil eine vorhandene Drehung durch die superponirte Doppelbrechung geschwächt wird und bis zur Unmerklichkeit verdeckt werden kann."

In the second part he communicates the repetition of Villari's experiment on the rotation of the plane of polarisation of light in a disk spinning between magnet poles. He finds a diminution of rotation from 5·06° to 0·77° when the disk spins 10,800 times in a minute, and proves experimentally that this is due to the double refraction produced by centrifugal force. For, on the one hand, the double refraction produces a difference of phase in the linear components, which, like the centrifugal force, is proportional to the square of the number of revolutions, and hence must be due to this force; on the other hand, the rotation vanishes as the theory requires, just where the difference of phase of the linear components is π.

Thus Mr. Ward's essential results, namely, the diminution of the rotation by double refraction, and the explanation of Villari's experiment by means of the double refraction due to centrifugal force, have been already communicated in the above-named papers.

With regard to Mr. Ward's mathematical deductions, the first of the before-mentioned authors wishes to point out a mistake which Mr. Ward has made in forming his differential equation.

In accordance with Mr. Ward, we let the x or y axis fall in the plane of principal section, and the z axis in the direction of the rays of light. We must examine how an ellipse is altered under the common action of rotation and double refraction, if we advance in the medium a short distance dz. Like Mr. Ward, we take the ellipse as produced by composition of y and x components, the ratio of whose amplitudes is $\tan\alpha$, and whose difference of phase is β; then the direction of the major axis forms with the x axis an angle ω, determined by the equation

$$\tan 2\omega = \tan 2\alpha \cos\beta. \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (1.)$$

Since the double refraction does not alter the value of α, we obtain the variation of ω with β under the influence of double refraction alone by forming $\partial\omega/\partial\beta$, regarding α as constant—

$$\frac{\partial\omega}{\partial\beta} = -\tfrac{1}{2}\cos^2 2\omega \tan 2\alpha \sin\beta,$$

or

$$\frac{\partial\omega}{\partial\beta} = -\tfrac{1}{4}\sin 4\omega \tan\beta. \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (2.)$$

So far our calculation agrees with Mr. Ward's.

To continue: let β vary under the influence of *double refraction* alone by $\partial\beta_\delta = k\,\partial z$, when the vibration advances through ∂z; then the alteration of ω is $\partial\omega = -\frac{1}{4}k \sin 4\omega \tan\beta\,\partial z$.

Under the influence of *rotation* alone, let ω vary by $\partial\omega_r = m\,\partial z$; then the total alteration of ω with z is given by the equation—

$$\frac{d\omega}{dz} = -\frac{k}{4}\sin 4\omega \tan\beta + m. \quad \ldots\ldots\ldots\ldots\ldots (3.)$$

In this equation β is still unknown; it is also variable with the double refraction and with the rotation. We must therefore form a second differential equation for β. We already know the alteration of β due to *double refraction* alone; it is $\partial\beta_\delta = k\,\partial z$. In order to learn the variation of β with *rotation* alone, we are in need of a relation between β and ω, in which only β and ω are variable in consequence of rotation. This relation is—

$$\tan\beta \sin 2\omega = \mathrm{K}, \quad \ldots\ldots\ldots\ldots\ldots (4.)$$

where

$$\mathrm{K} = -\frac{2k}{1-k^2},$$

if k is the ratio of the minor and major axes of the ellipse. This ratio, as a matter of fact, does not alter with rotation.

From this follows—

$$\frac{\partial\beta}{\partial\omega} = -\frac{\sin 2\beta}{\tan 2\omega}. \quad \ldots\ldots\ldots\ldots\ldots (5.)$$

But since $\partial\omega_r = m\,\partial z$, the variation of β by rotation alone is $\partial\beta_r = -m\dfrac{\sin 2\beta}{\tan 2\omega}\partial z$. The total variation of β with z is hence given by the equation—

$$\frac{d\beta}{dz} = k - m\frac{\sin 2\beta}{\tan 2\omega}. \quad \ldots\ldots\ldots\ldots\ldots (6.)$$

Thus the complete solution of the problem is contained in the two simultaneous differential equations—

$$\left.\begin{aligned} \frac{d\omega}{dz} &= -\frac{k}{4}\sin 4\omega \tan\beta + m \\ \frac{d\beta}{dz} &= k - m\frac{\sin 2\beta}{\tan 2\omega}. \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots (7.)$$

Mr. Ward's solution, however, consists in the single differential equation—

$$\frac{d\omega}{dz} = -\frac{k}{4}\sin 4\omega \tan kz + m. \quad\ldots\ldots\ldots\ldots\ldots (8.)$$

He obtains it by setting in equations (2) and (3) $\beta = k.z$. This relation is not correct, since it only represents the variation of β with double refraction, whilst β also varies with rotation.

To show the effect of this mistake, we will consider the particularly simple case where the incident vibration is along the x axis alone, and where the double refraction is so much stronger than the rotation that we can replace $\sin\omega$ by ω.

The correct differential equations are—

$$\left.\begin{aligned}\frac{d\omega}{dz} &= -k\omega\tan\beta + m\\ \frac{d\beta}{dz} &= k - \frac{m}{2\omega}\sin 2\beta.\end{aligned}\right\} \quad\ldots\ldots\ldots\ldots\ldots (9.)$$

Mr. Ward's is—

$$\frac{d\omega}{dz} = -k\omega\tan kz + m. \quad\ldots\ldots\ldots\ldots\ldots (10.)$$

The solution of the simultaneous differential equations (9) is—

$$\left.\begin{aligned}\omega &= \frac{m}{k}\sin kz\\ \beta &= \frac{k}{2}z,\end{aligned}\right\} \quad\ldots\ldots\ldots\ldots\ldots (11.)$$

whilst Mr. Ward obtains for this case by integration of (10)—

$$\omega = \frac{m}{k}\cos kz \log_e \tan\left(\frac{\pi}{4}+\frac{kz}{2}\right). \quad\ldots\ldots\ldots\ldots (12.)$$

From (11) it follows in accordance with experiment that $\omega = 0$ when kz, the difference of phase, is a multiple of π. Mr. Ward concludes from (12) that $\omega = 0$ when kz is a multiple of $\pi/2$.

To conclude: Mr. Ward's single differential equation must be replaced by two simultaneous differential equations. The conclusions which Mr. Ward draws from his equation are hence in part incorrect, in part not properly proved.

[Reference may also be made to Professor Willard Gibbs's investigation of "Double Refraction in perfectly Transparent Media which exhibit the Phenomena of Circular Polarisation," 'Amer. Journ. Sci.,' June, 1882.—R.]

II. "Researches on the Chemistry of the Camphoric Acids." By J. E. MARSH, B.A., Demonstrator of Organic Chemistry at the University Laboratory, Oxford. Communicated by Professor ODLING, F.R.S. Received August 15, 1889.

For some time past I have been engaged in the study of some derivatives of camphor, with the view of determining, if possible, with some degree of certainty the constitution of that body. I do not propose to enter now into any account of the present state of our knowledge as to the chemistry of camphor, but at once to bring forward something of my own experience on this subject.

Of part of the results which I have obtained I gave a brief account at the meeting of the British Association last year at Bath. This account had reference chiefly to some further evidence of the relationship of camphor to hexahydrometaxylene, and to the products of oxidation of camphoric acid by means of an alkaline solution of potassium permanganate. These researches were not published, and are not even yet completed.

On the other hand, I have made some additional experiments which I am anxious to bring forward now, as it may be some time before the whole series is finished.

The experiments to be described have reference to certain processes which resulted in the preparation of a new camphoric acid; and the remarkable properties of this acid contrasted with those of the ordinary camphoric acid throw considerable light on the nature of the isomerism subsisting between these bodies.

Camphoryl Chloride.

I have had occasion to prepare considerable quantities of camphoryl chloride, $C_{10}H_{14}O_2Cl_2$, by the action of pentachloride of phosphorus on ordinary camphoric acid. Camphoryl chloride was obtained originally by Moitessier ('Compt. Rend.,' vol. 52, p. 871), but only as a crude product, which he found to decompose on distillation. The substance may, however, be readily purified by distillation *in vacuo*, when it distils about 140° C. under 15 mm. pressure. There appears to be no difference in the product of the reaction however much pentachloride of phosphorus is used, provided the temperature be not raised above that of boiling water.

Chlorocamphoryl Chloride.

If, however, camphoric acid is heated with a large excess of pentachloride of phosphorus on a sand-bath in a flask provided with a reflux condenser, a new body of the formula $C_{10}H_{13}Cl_3O_2$, viz., chloro-

camphoryl chloride, is obtained. A further account of this body and its transformations I will reserve for a future publication.

The camphoryl chloride was prepared in order to attempt to reduce it to the lactone similar to the lactones obtainable from phthalyl and succinyl chlorides. It was not found possible to obtain a lactone under the various conditions of experiment from time to time adopted.

Action of Water on Camphoryl Chloride.

Circumstances, however, led to the consideration of a more simple reaction, namely, the action of water on camphoryl chloride. Moitessier had stated that this reaction was such as to give back camphoric acid. He means, presumably, the original dextro-rotatory acid, as he does not mention more than one. This acid is, in fact, formed, as will be shown subsequently, but only in very small quantity, and can only be separated from and recognised among the other products of the reaction after special treatment.

If camphoryl chloride obtained from ordinary dextro-camphoric acid is added gradually to about ten times its weight of hot water, there is formed about equal quantities of camphoric anhydride and a new camphoric acid, which rotates the ray of polarised light to the *left*. These two bodies are contained in the precipitate after cooling, and form by far the greater part of the product. Besides these, there is formed a small quantity of another substance more soluble in water, which separates from a strong hot aqueous solution as an oil, after a time solidifying. This is a mixture of the ordinary dextro-camphoric acid with the new lævo-rotatory acid. I will refer to this again later.

The camphoric anhydride and lævo-camphoric acid obtained in this reaction are readily separated by treatment with carbonate of soda in the cold, by means of which the lævo-acid is dissolved, leaving the anhydride untouched. This anhydride is converted into the ordinary dextro-rotatory camphoric acid by solution in hot caustic soda and precipitation by hydrochloric acid. I have been able to confirm de Montgolfier's observation that the camphoric acid obtained from the anhydride has a specific rotatory power of over $+48°$. I have found $[\alpha]_D = +48{\cdot}25°$. (The determinations of rotatory power have been made throughout with a Laurent half-shade polarimeter, kindly lent me by Dr. Haldane, of the Physiological Department.)

Properties of the Lævo-camphoric Acid.

The lævo-rotatory acid obtained above has a rotation about equal, and of opposite sign to the dextro-acid. I have found $[\alpha]_D = -48{\cdot}09°$. I am convinced, however, that these two acids are not optically

opposite isomers, in the sense namely in which dextro- and lævo-tartaric acids are regarded as optically opposite.

In its ordinary properties the lævo-acid differs markedly from the dextro-isomer. It has a lower melting point, namely, 170° C., as compared with 185 C°. Again it yields a very soluble amorphous barium salt, the corresponding salt of the dextro-acid being also indeed very soluble but crystalline. A more remarkable distinction, however, is that the lævo-acid appears to have no corresponding anhydride. When subjected to the action of acetyl chloride, an important reagent employed by Anschütz for obtaining the anhydrides of dibasic acids, the dextro-acid readily and practically quantitatively yields its anhydride, which is insoluble in carbonate of soda. The lævo-acid, on the other hand, when treated in the same manner, yields a product almost completely soluble in carbonate of soda, from which the original acid of melting point 170° is recovered apparently unaltered.

Conversion of the Lævo- into the Dextro-Acid.

If, however, the lævo-acid be distilled, it boils about 294° C., yielding the anhydride of the ordinary dextro-camphoric acid, from which anhydride this acid may be obtained in the ordinary way.

Thus it appears possible to convert any quantity of the dextro-acid almost entirely into the lævo- through the intervention of the chloride, and any quantity of the lævo- back again to the dextro- by means of the anhydride.

Mixture of the two Acids.

Chautard obtained a lævo-camphoric acid by the oxidation of a lævo-rotatory camphor. This acid is described as possessing all the properties of ordinary camphoric acid, except that its rotatory power is of opposite sign. Further, when strong alcoholic solutions of the two acids are mixed there is a precipitation of crystals and a rise of temperature in the liquid.

The lævo-acid which I have obtained differs from Chautard's in other properties, and also in this, that when mixed with the dextro-acid in equal quantities in strong alcoholic solution there is no crystalline deposit and no rise of temperature.

On boiling down the alcoholic solution, a syrupy residue is left, which dissolves in hot water more readily apparently than either of its constituents. From the aqueous solution it separates partly as an oil, which after a time solidifies, and partly as crystals. Neither the solidified oil nor the crystals have a definite melting point, but both melt at a lower temperature than either of the component acids. Hence the two acids do not appear to form any definite compound camphoric acid.

Separation of the Mixed Acids.

Anschütz's acetyl chloride reaction furnishes a particularly neat method of separating the two acids. The anhydride of the dextro-acid formed by the action of the acetyl chloride is separated from the apparently unaltered lævo-acid by treatment with carbonate of soda, which dissolves only the latter.

Nature of the more soluble Syrupy Product of the Action of Water on Camphoryl Chloride.

This substance, which resembled the mixture of dextro- and lævo-acids above described, was subjected to the same process of separation, by which exactly the same products were obtained.

Thus the action of water on the acid chloride is such as to give about equal quantities of lævo-camphoric acid and the anhydride of dextro-camphoric acid, a small portion of latter being further converted into the acid.

Optical Activity of Camphoryl Chloride and Camphoric Anhydride.

It might be supposed, from the circumstances just mentioned, that camphoryl chloride which yields both dextro- and lævo-camphoric acids would be itself inactive. This, however, is not the case. I have always found camphoryl chloride to be lævo-rotatory, though I have not found a perfectly constant number to express its value. For the undiluted substance I have found $[\alpha]_D = -3{\cdot}0°$, and $-3{\cdot}6°$, while for the substance dissolved in crude benzene the rotation is about twice as much, viz., $[\alpha]_D = -7{\cdot}1°$ and $-8{\cdot}3°$; the latter value being obtained with the same specimen of camphoryl chloride which gave a rotation of $3{\cdot}6°$ when undiluted. The rotation is, indeed, greater than that of camphoric anhydride, a substance which yields only one camphoric acid. I have found the specific rotation of camphoric anhydride in pure benzene to be $-3{\cdot}7°$, and very little different in the same benzene which was employed for the camphoryl hloride. De Montgolfier has given a higher value, $-7° \, 7'$, for camphoric anhydride, which I am unable to confirm.

Theoretical Considerations.

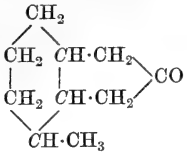
If we wish to interpret rightly the foregoing facts, we shall, I think, find it impossible to regard the lævo-camphoric acid as the optically opposite isomer of the ordinary acid. On the other hand, there would appear to be a probability, almost amounting to certainty, that these two acids are related to one another as maleïc and fumaric acids, the dextro-camphoric acid corresponding to maleïc and the lævo- to fumaric acid. The isomerism is further complicated by the presence of rotatory power which requires the existence of two other

isomers which shall be the optically opposite isomers of these two. In this latter case, therefore, the two acids will also be related, as maleïc and fumaric acids, but the lævo-acid will correspond to maleïc and the dextro- to fumaric. This lævo-camphoric acid is without doubt the acid which Chautard obtained by the oxidation of lævo-camphor, and the fumaroid dextro-acid I hope to obtain from it by the hydrolysis of the acid chloride.

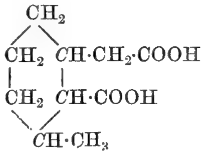
Now, accepting unreservedly Van't Hoff's carbon-atom hypothesis, we find that the isomerism of the camphoric acids is not explained by different structural formulæ, but rather by one and the same structural formula, admitting of at least four different arrangements of the atoms in space. There will be then two conditions attached to such a structural formula: (1) Either it will contain two doubly-linked carbon-atoms, each of them being united to two different groups, and one of these groups containing also an asymmetric carbon-atom; (2) or the formula may consist of a closed chain of carbon-atoms, of which two or more are asymmetric. In the latter case, it is necessary to mention a further restriction, namely, that when there are only two asymmetric carbon-atoms in the ring, there must not be symmetry in the formula, or, in other words, the image of neither isomer must be identical with its object (see 'Phil. Mag.,' 1888, vol. 26, p. 426). It is the more necessary to mention this restriction as it applies to Wreden's formula, a formula which does not admit of rotatory power in the maleïnoid acid, that is, in the ordinary camphoric acid.

Turning now to some of the formulæ proposed for camphoric acid by different chemists, we find that in general they satisfy one or other of the conditions before mentioned. But Kekulé's formula, which fulfils the first condition, and Kachler's, which satisfies the second, cannot be accepted, on the ground that they do not represent camphoric acid as a derivative of hexahydrometaxylene. Wreden's formula which does thus represent camphoric acid does not, as I have just mentioned, satisfy the restricted second condition. Armstrong's formulæ satisfy the second condition, and also represent camphoric acid as a derivative of hexahydrometaxylene, but Armstrong's formulæ appear to me liable to objection on the ground of the formula for camphor from which they are derived. These formulæ represent camphor either as not containing a ketonic group, or if it contain a ketonic group as containing also a closed chain of four carbon-atoms. Now, we know no instances of stable rings of four carbon-atoms, if indeed we know instances of such rings at all; and it is unlikely that camphor is the one exception to what may be regarded as a general rule.

The formulæ which I have proposed for camphor and camphoric acid appear to be opposed by no facts, and also explain perfectly the isomerism of the camphoric acids. Camphor is represented by the formula—

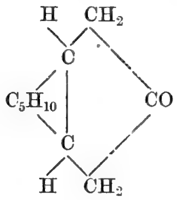


and camphoric acid by the formula—

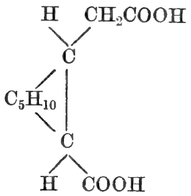


(The asymmetric carbon-atoms are in italics).

Now the ordinary camphoric acid being the maleïnoid acid has the two groups (COOH) and (CH_2·COOH) on the same side of the ring plane of the six carbon-atom nucleus. Hence in camphor, as we should naturally have supposed, the grouping, $CO<\begin{matrix}CH_2—\\CH_2—\end{matrix}$, is also attached on one side of and not across the ring plane—

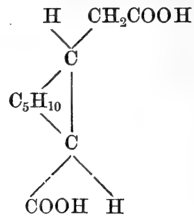


(dextro- and lævo-camphor).



(dextro- and lævo-ciscamphoric acid).

Again the lævo-fumaroid acid has the carboxyl and (CH_2COOH) groups on opposite sides of the ring plane.



(dextro- and lævo-transcamphoric acid).

On the other hand, Chautard's lævo-camphoric acid, being obtained directly from a lævo-camphor, will have the respective groups on the same side.

Nomenclature.

We may adopt the nomenclature introduced by von Baeyer to distinguish the hydroterephthalic acids. Ordinary camphoric acid then is dextro- and Chautard's acid lævo-ciscamphoric acid, the prefix *ci* indicating that the carboxylic groups are on the same side of the ring plane.

The new acid again, which I have described in this paper, is the lævo- and the acid yet to be discovered will be the dextro-transcamphoric acid, the prefix *trans* implying that the groups which govern the isomerism are on opposite sides of the ring plane.

It will be noticed that the formulæ which I have adopted require the existence of another series of two camphors (dextro- and lævo-) and four camphoric acids; but of the existence of these we have as yet no evidence.

[*Postscript, Dec. 2nd,* 1889.—Since the above was written, my attention has been drawn to a paper by M. Friedel in the 'Comptes Rendus' of last May, in which he describes a lævo-camphoric acid obtained from the substance known for some time as mesocamphoric acid. This lævo-acid appears to be the same as the one I have just described, and the mesocamphoric acid to be a mixture of the lævo- with the original dextro-acid. Thus mesocamphoric acid must no longer be regarded as a distinct isomer.]

III. "The Influence of Stress and Strain on the Physical Properties of Matter. Part III. Magnetic Induction (*continued*). The Internal Friction of Iron, Nickel, and Cobalt, studied by Means of Magnetic Cycles of very minute Range."* By HERBERT TOMLINSON, B.A., F.R.S. Received August 22, 1889.

(Abstract.)

The author has already studied the internal friction of metals by the method of torsional oscillations, and deduced certain simple laws relating thereto. One of the principal objects of the present enquiry was to ascertain how far the dissipation of energy resulting from statical molecular friction which occurs in magnetic cycles of very minute range would be amenable to the laws of dissipation of energy occurring in torsional cycles. The "ballistic" method of observation has been employed, the arrangements being exceedingly sensitive so as to admit of the use of very feeble magnetising forces.

The following is a summary of the results obtained:—

1. When a rod of iron, nickel, or cobalt is made to pass through a complete magnetic cycle of sufficiently minute range of magnetising force, the changes of magnetic induction can be expressed by an equation of the form—

$$y = Ax + Bx^2,$$

where x represents the change of magnetising force as the force passes from its greatest value in the cycle to zero, if the force be always applied in the same direction, or as the force passes from its greatest value in the one direction to its greatest value in the opposite direction, if the force be alternately applied in one direction and in the opposite, y represents the corresponding change of magnetic induction, and A and B are constants throughout the cycle.

2. The values of A and B are the same for magnetic cycles of different ranges of magnetising force, provided the force does not exceed certain limits. These limits vary considerably with the nature of the metal and with the mechanical or thermal treatment to which it may have been subjected.

3. The constant A is a measure of the magnetic permeability of the metal when the magnetising force is infinitesimal. The product of B and the cube of the maximum force in the cycle is a measure of

* In prosecuting this research the author has received assistance from the Elizabeth Thompson Science Fund, U.S.A., and from the English Government Grant Fund.

the dissipation of energy, arising from statical friction among the molecules, in the performance of the cycle.

4. The internal friction of iron, nickel, and cobalt in any complete cycle may be decreased by repetition of the cycle; the molecules are said to be "accommodated" by this process.

5. The molecular "accommodation" of freshly-annealed iron can be largely aided by repeatedly raising the metal to 100° C. and then allowing it to cool.

6. The "accommodation" of the molecules of iron, nickel, and cobalt is disturbed by very slight mechanical shocks, by small change of temperature, or by magnetisation beyond certain limits; under such influences the internal friction may for a time, or even permanently, be considerably increased.

7. The values of A and B for iron, nickel, and cobalt can be very largely decreased by permanent mechanical strain. They may also be largely decreased in the case of steel by sudden cooling from a high temperature. No amount of strain, however, whether produced by mechanical or thermal agency, can reduce either the magnetic permeability or the internal friction below certain limits.

8. The values of A and B for iron are temporarily increased by loading not carried beyond a certain limit; beyond this limit both A and B decrease with increase of load.

9. The values of A and B for iron, nickel, and cobalt are temporarily increased when the temperature is raised from 0° C. to 100° C.

The amount of increase is much greater in freshly-annealed iron wire than in the same wire after it has been repeatedly raised to 100° C. and then cooled.

10. In both torsional and magnetic cycles of sufficiently minute range, the dissipation of energy from internal friction is independent of the *rate* at which the cycle is performed.

11. In both torsional and magnetic cycles of given range of force the average dissipation of energy per cubic centimetre does not depend upon the *dimensions*, provided that in magnetic cycles the length is sufficiently great to permit of neglecting the effect of the ends.

12. It follows from 3 that the dissipation of energy in a *magnetic* cycle is proportional to the *cube* of the maximum magnetising force in the cycle. On the contrary, the dissipation of energy in a *torsional* cycle is proportional to the *square* of the maximum torsional force provided the force does not exceed certain limits.

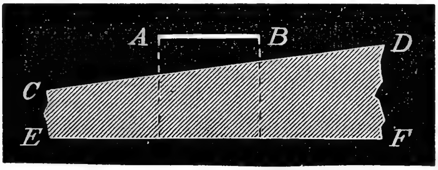
13. As regards "accommodating" influences and also as regards those influences which disturb "accommodation," the dissipation of energy in a magnetic cycle seems to be strictly analogous to the dissipation of energy in a torsional cycle. But the temporary and permanent effects of mechanical stress and the temporary effects of change of temperature are very different in the two kinds of cycles.

IV. "A Compound Wedge Photometer." By E. J. Spitta, L.R.C. Phys. Lond. Communicated by Capt. W. de W. Abney, C.B., F.R.S. Received November 8, 1889.

The idea of employing a wedge of neutral-tinted glass as a photometer has occurred to many observers—Dawes, Captain Abney, and others—and notably of late years to Professor Pritchard, of Oxford, who has produced with such an instrument his well known 'Uranometria Nova Oxoniensis,' a catalogue of the relative brightness of the brighter stars north of the equator. But the use of such an instrument has always been limited hitherto to the comparison of the relative intensities of such points of light as the stars present, its employment upon objects of sensible area being foreign to the ideas and requirements proposed in its construction. Having, however, attempted to use a photometer of this description upon disks of small but of various areas illuminated by a known amount of light, the discordances of the results forced upon me the necessity of modifying the construction of the photometer in a way which I believe will extend its sphere of usefulness. It is not within the scope of this paper to give any detailed account of the many experiments I have made with several wedges, but it is sufficient to say that the wedge-form itself has been fully proved to be an important factor in the production of the discordances to which reference has been made, for the following reasons:—

A point of light from its very definition implies that no sensible portion of the wedge is occupied in its passage, but it requires very little thought to perceive that when an area of sensible dimensions is being dealt with this is by no means the case. Moreover, an elementary inquiry suffices to point out that if the area possess a considerable diameter the light emanating from its lateral portions will impinge on different thicknesses of the wedge, as shown to an exaggerated degree in fig. 1, where AB is the transverse diameter of

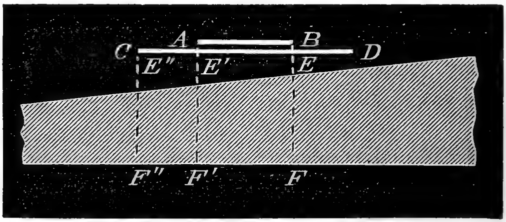
Fig. 1.



the area, and CDEF the portion of the wedge employed. It is evident if the differences in intensity be required between two disks

of the same diameter this condition of things would not affect the validity of the final results, but it is equally apparent that were the disks of differing diameter the values obtained could not but be seriously affected. Let it be presumed that two such different-sized disks were under consecutive examination, as shown in fig. 2, AB

FIG. 2.



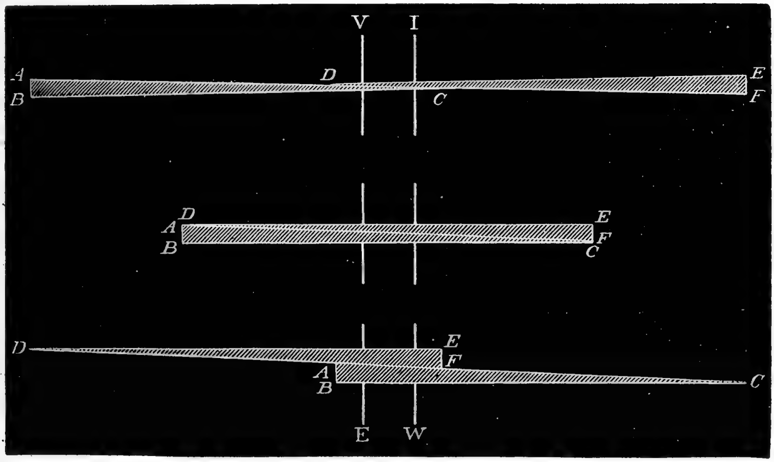
representing the diameter of one and CD that of the other. In the case of AB it is manifest that the extreme limb B would be fainter to all appearance than the opposite edge A, because the light issuing from it has to traverse a portion of the wedge EF, thicker and so more dense than E′F′. On consideration, it is equally obvious that the limiting margin A will be the last to appear as the wedge is made to move from right to left till disappearance takes place, the position technically spoken of as "the point of wedge extinction." But if CD, the larger disk, be illuminated with light of the same initial intensity as fell on AB, it is evident that the point of wedge extinction (technically so-called) for CD at the limb C will not be at the same wedge-reading as in the previous case; in fact, disappearance would not occur until the portion of the wedge corresponding to the line E′F′ had been moved to occupy the position shown by the line E″F″. Hence, when ascertaining what is termed the wedge-interval in the direct comparison, say, of the relative intensities of a large and small disk, it is very obvious an error entirely due to the physical nature of a wedge must inevitably result, such error being in direct proportion to the amount of shift required, which depends upon the relative differences in diameter of the disks under observation. Nothing is here said of the difficulties of observation, which are enormously increased by the different apparent intensities of the light at the extremities of the diameter parallel to the length of the wedge, because I merely wish to call attention to the error resulting from the employment of the wedge-form itself.

To apply a correction under these circumstances was not deemed expedient, even if found to be practically possible; hence the removal of the source of error has been arrived at by devising an instrument of

different construction, to which the term Compound Wedge Photometer has been applied, and of which the following is a brief description :—

Two very thin wedges of neutral-tinted glass are made to slide past one another with a uniform rate of motion by the turning of a single milled-headed screw, the idea of the arrangement being diagrammatically set forth, so far as the wedges themselves are concerned, in fig. 3, where it will be seen that any amount of density, within

Fig. 3.



certain limits, can be obtained by equal movement of the two wedges, although a uniformly absorptive area in all parts of the field is rigidly maintained. In the figure, ABC is shown as one wedge, DEF the other, and VIEW the field of view. A cursory inspection of the arrangement at once reveals its most salient advantages, and the fact that any sized disk within the limits of the field of vision will be obscured by the same density of neutral-tinted glass at any and all parts of its image, and hence that the cause of error spoken of as arising from the use of a single wedge is at once removed.

An instrument so constructed has been subjected to several months' crucial testing, and I have no grounds for thinking it does not fulfil the requirement for which it was devised. In its final form the arrangement differs from that usually met with as suggested by Professor Pritchard, for it is supplied with a rotating disk of metal, perforated at intervals to allow the permanent insertion of pieces of neutral-tinted glass of different thickness, each of which can be evaluated for magnitude and used as a constant. Besides, it is

fixed upon the occulting eye-piece, a device for limiting the aperture at the eye-end of a telescope, or for occulting any portion or portions of the field of view, and which I have fully described in vol. 45 of the 'Monthly Notices of the Royal Astronomical Society.'

Presents, December 5, 1889.

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Framed Engraved Portrait of Brook Taylor, LL.D. (Sec. R. S., 1714). Prof. Greenhill, F.R.S.

December 12, 1889.

Sir G. GABRIEL STOKES, Bart., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "The Relation of Physiological Action to Atomic Weight." By Miss H. J. JOHNSTONE, University College, Dundee, and THOS. CARNELLEY, Professor of Chemistry in the University of Aberdeen. Communicated by Sir HENRY ROSCOE, F.R.S. Received October 5, 1889.

II. "An Experimental Investigation into the Arrangement of the Excitable Fibres of the Internal Capsule of the Bonnet Monkey (*Macacus sinicus*)." By CHARLES E. BEEVOR, M.D., F.R.C.P., and VICTOR HORSLEY, B.S., F.R.S. (From the Laboratory of the Brown Institution). Received December 4, 1889.

(Abstract.)

After a historical introduction the authors proceed to describe the method of investigation, which was conducted as follows. The animal being narcotised with ether, the internal capsule was exposed by a horizontal section through the hemisphere. By means of compasses the outlines of the basal ganglia and capsule were accurately transferred to paper ruled with squares of 1 millimetre side, so that a projection of the capsule was thus obtained, divided into bundles of 1 millimetre square area. Each of these squares of fibres was then excited by a minimal stimulus, the same being an induced or secondary interrupted current. The movements were recorded and the capsule photographed.

In all 45 experiments were performed, and they are arranged in eight groups, representing eight successive levels (*i.e.*, from the centrum ovale to the crus) at which the capsule was investigated.

Before the results are described in detail a full account is given of previous investigations, experimental, clinical, and anatomical, on the arrangement of the internal capsule.

The anatomy of the part and the relation of the fibres to the basal ganglia are then discussed, and a full description given of each of the groups examined.

The general results are next given at length, of which the following is a *résumé.*

Firstly, the rare occurrence of bilateral movement is discussed, and the meaning of the phenomenon defined. Secondly, the lateral arrangement and juxtaposition of the fibres are considered. Thirdly, the antero-posterior order in which the fibres for the movements of the different segments are placed is described, and that order found to be practically identical with that observed on the cortex, viz., from before back.

Movements of eyes.
,, head.
,, tongue.
,, mouth.
,, upper limb (shoulder preceding thumb).
,, trunk.
,, lower limb (hip preceding toes).

The character or nature of these movements is set out in a table giving the average localisation of each segment. Speaking generally, it may be said that the movements are arranged in the same way as has already been shown by the authors to exist in the cortex (*vide* previous papers in 'Phil. Trans.,' 1887, 1888), viz., that the representation of extension is situated in front of that of flexion for the segments of the upper limb, while for the toes flexion is obtained, as in the cortex, in front of extension.

Numerous tables and diagrams are appended, showing the extent of appropriation of fibres for each movement.

III. "On the Effect of the Spectrum on the Haloid Salts of Silver." By Captain W. de W. Abney, C.B., R.E., D.C.L., F.R.S., and G. S. Edwards, C.E. Received November 26, 1889.

[Publication deferred.]

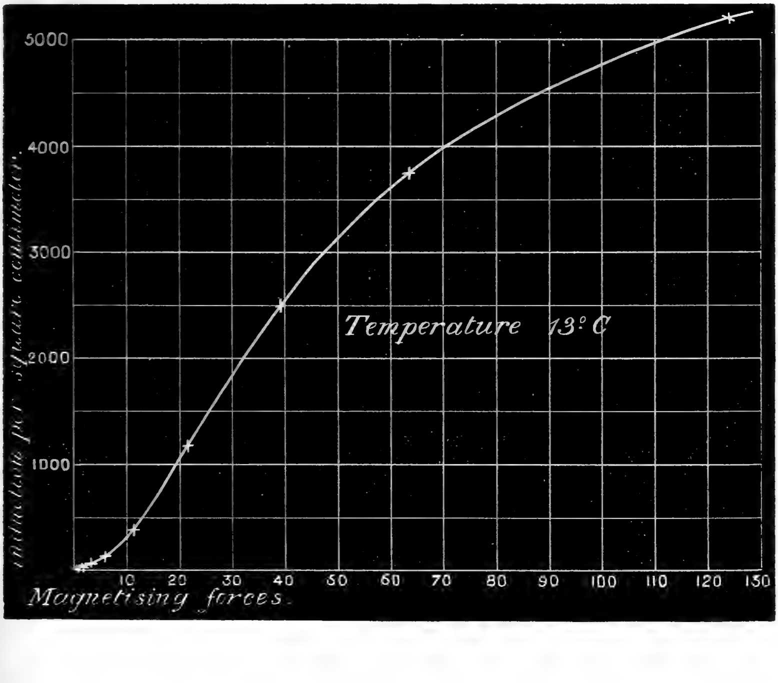
IV. "Magnetic Properties of Alloys of Nickel and Iron." By J. HOPKINSON, D.Sc., F.R.S. Received December 2, 1889.

Several alloys have been examined, supplied to me very kindly by Mr. Riley, of the Steel Company of Scotland. I confine myself to a brief statement of the results with the most interesting sample. Mr. Riley informs me that this sample contains 25 per cent. of nickel. As the material was given to me it was non-magnetic at ordinary temperature, that is to say, the permeability was small, about 1·4, and the induction was precisely proportional to the magnetising force. The ring on being heated remained non-magnetic up to 700° C. or 800° C. A block of the material did not recalesce on being heated to a high temperature and being allowed to cool.

On being placed in a freezing mixture the material became magnetic at a temperature a little below freezing point.

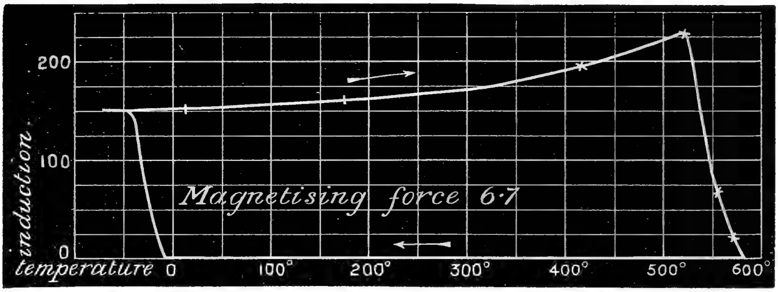
The material was next cooled to about −51° C., by means of solid carbonic acid, and the curve of magnetisation was ascertained, as shown in fig. 1, corresponding to a temperature of 13° C.; from this it will be seen that the ring of the material, which was previously

FIG. 1.



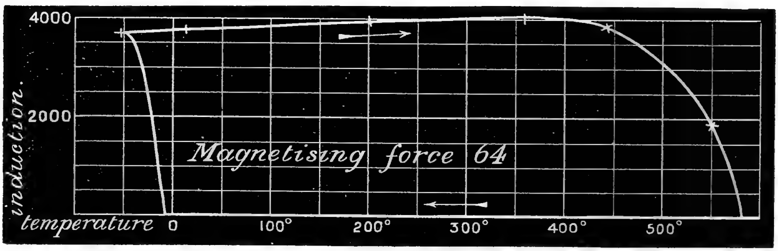
non-magnetic at 13° C., is now decidedly magnetic at the same temperature. On heating the material it remained magnetic until it reached a temperature of 580° C. At this temperature it became non-magnetic, and, on cooling, remained non-magnetic to the ordinary temperature of the room. Fig. 2 shows the inductions at various

FIG. 2.



temperatures, the abscissæ being temperatures for a magnetising force 6·7, whilst fig. 3 shows the induction in terms of the temperature to a different scale for a force of 64.

FIG. 3.



These curves show that, for a range of temperature from somewhat below freezing to 580° C., this material exists in two states, either being quite stable, the one being non-magnetic, the other magnetic. It changes from non-magnetic to magnetic if the temperature be reduced a little below freezing; the magnetic state of the material does not change from magnetic to non-magnetic till the temperature is raised to 580° C.

The same kind of thing, in a much less degree, can be seen with ordinary steel. Over a small range this can exist in two states, but in changing its state from non-magnetic to magnetic a considerable amount of heat is liberated, which causes a rise of temperature of the steel. Whether any material quantity of heat is latent in the nickel steel I do not know.

Presents, December 12, 1889.

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December 19, 1889.

Sir G. GABRIEL STOKES, Bart., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Comparison of the Spectra of Nebulæ and Stars of Groups I and II with those of Comets and Auroræ." By J. NORMAN LOCKYER, F.R.S. Received November 9, 1889.

The discussion of cometary spectra which was communicated to the Royal Society in November, 1888,* contained, among other matters, conclusions which have a special bearing on the relations of their spectra to those of other bodies.

I have thought it therefore desirable to bring this new material together with the more complete lists of lines which are now available in the case of other groups of celestial bodies, chiefly owing to the observations made by my assistants and others and myself since my paper of 1887 was written, but also in consequence of a more complete search among previously recorded observations.

Such a comparison—a much more complete one than was possible in the first instance—would strengthen or weaken my hypothesis according as the increased area of observation increased or decreased the number of coincidences in the spectra of the various groups.

The more the coincidences are intensified the greater is the probability that comets, nebulæ, stars with bright lines, stars with mixed flutings, and the aurora have a common origin, independent of the chemical origins which have been assigned to the various lines by laboratory observations.

In the tables which follow, the individual observations are not given, but under each heading all the lines or flutings which have been recorded find place.

I. COMPARISON OF COMETS AND NEBULÆ.

We may conveniently begin with a comparison of comets and nebulæ. The Great Comet of 1882 and Comet Wells, when near perihelion, are excluded from the list of cometary lines and flutings, as their temperature was too high for fair comparison with most of the nebulæ and other low temperature phenomena.

* 'Roy. Soc. Proc.,' vol. 45, pp. 159—217.

In cases where any of these higher temperature lines correspond to lines in the comparison spectrum, however, they have been added to the list of cometary lines, in brackets, as sometimes the phenomena compared may attain a temperature slightly higher than that of comets at mean temperature.

For the nebulæ, all the lines recorded in the visible spectrum by Messrs. Huggins, Vogel, Copeland, Taylor, and Fowler are given. The list of lines has been considerably extended since my preliminary discussion of the spectra of nebulæ in November, 1887. D_3 and a line at 447 have been observed in the spectrum of the nebula in Orion by Copeland, and Mr. Taylor has also recorded D_3 and lines, or remnants of flutings, at 559 and 520. In the nebula in Andromeda, carbon flutings and the lead flutings at 546 have been observed by Mr. Fowler and confirmed by Mr. Taylor; since these observations were made, I find that Vogel* has observed a line at 518, probably carbon 517, in nebulæ numbered in Sir J. Herschel's General Catalogue 4234, 4373, and 4390.

Other nebula lines with which I was not previously acquainted are 479, 509, and 554. All these lines were observed by Vogel in the nebula G.C. 4378.†

With reference to the appearance of D_3 in nebulæ and bright-line stars, I wrote, in November, 1887‡:—"It is right that I should here point out that some observers of bright lines in these so-called stars have recorded a line in the yellow which they affirm to be in the position of D_3; while, on the other hand, in my experiments on meteorites, whether in the glow or in the air, I have seen no line occupying this position.

"I trust that some observer with greater optical means will think it worth his time to make a special inquiry on this point. The arguments against this line indicating the spectrum of the so-called helium are absolutely overwhelming. The helium line so far has only been seen in the very hottest part of the Sun which we can get at. It is there associated with *b*, and with lines of iron which require the largest coil and the largest jar to bring them out, whereas it is stated to have been observed in stars where the absence of iron lines and of *b* shows that the temperature is very low. Further, no trace of it was seen in Nova Cygni, and it has even been recorded in a spectrum in which C was absent, and once as the edge of a fluting.§

"It is even possible that the line in question merely occupies the

* 'Bothkamp, Beob.,' Heft 1, 1872, p. 57.

† 'Bothkamp, Beob.,' Heft 1, 1872, p. 57.

‡ 'Roy. Soc. Proc.,' vol. 43, p. 139.

§ ". . . . The spectrum is very bright: two strong bands are seen in the red, then the D line, followed by a bright line (D_3) as the edge of a band . . . ." (Konkoly, "Neuer Stern bei χ Orionis," 'Astr. Nachr.,' No. 2712).

position of D_3 by reason of the displacement of D by motion of the 'stars' in the line of sight. On this point no information is at hand regarding any reference spectrum employed.

"*If, however, it should eventually be established that the line is really D_3, which probably represents a fine form of hydrogen, it can only be suggested that the degree of fineness which is brought about by temperature in the case of the Sun, is brought about in the spaces between meteorites by extreme tenuity.*"

The observations of Dr. Copeland* have now, I think, established the identity of the yellow line, in the nebula of Orion at all events, with D_3. In a letter to Dr. Copeland, I suggested that the line at 447 was in all probability Lorenzoni's *f* of the chromosphere spectrum, seeing that it was associated both in the nebulæ and chromosphere with hydrogen and D_3. This he believes to be very probable. The line makes it appearance in the chromosphere spectrum about 75 times to 100 appearances of D_3 or the lines of hydrogen.

The association of the line at 447 with D_3 therefore strengthens the view that there is an action in space, away from condensations, whereby matter is reduced to its finest forms.

Comets.	Nebulæ.	Probable origins.	λ of probable origins.
—	411	H	4101
431	—	CH	431
—	434	H	434
—	447	?	—
468—474	468—474	C (hot)	468—474
—	479	?	—
483	—	C (cool)	483
486	486·3	H	486
—	495	?	—
500	500	Mg	500
—	509	?	—
517	517	C (hot)	517
519	—	C (cool)	519
521	520	Mg	521
[527]	527	Fe	527
546	546	Pb	546
—	554	?	—
558	559	Mn	558
561	—	C (cool)	561
564	—	C (hot)	564
568	—	Pb, Na	568
—	5872	? (D_3)	—

The table shows that there are many striking similarities between the two spectra, and there is no doubt that many of the lines are

* 'Monthly Notices, R.A.S.,' vol. 48, p. 360.

identical. The flutings of hot carbon, for example, are common to both, as are also the flutings of magnesium, manganese, and lead. The hydrogen line 486 has only been seen in one comet, namely, Comet III, 1880, by Konkoly.*

Other flutings and lines again are special to comets and others to nebulæ. Thus, there are practically no indications of hydrogen in comets, although the hydrogen lines are amongst the brightest in nebulæ. Again, the lines 447, 479, 495, 509, 554, and 5872 are seen in nebulæ, but not in comets. On the other hand, the cool carbon flutings and the fluting at 568 are seen in comets, but not in nebulæ. Most of these apparent discrepancies are explained by a consideration of the differences in the conditions of comets and nebulæ. It must be remembered that in the case of comets there is an action which repels the vapours produced by collisions, and the vapours first affected will, of course, be those which are least dense. Hydrogen will thus be repelled from the comets, whilst the denser vapours of magnesium and carbon remain. There is then a good reason why hydrogen lines should *not* be seen in cometary spectra. As there can be no such repulsion in the sparse swarms which constitute nebulæ, hydrogen lines are seen in them.

Two other lines special to nebulæ are 5872 and 447, to which reference has already been made. The evidence tends to show that D_3 and *f* are finer vapours than hydrogen, and hence there is even greater reason for the absence of these lines from cometary spectra, even were the temperature higher, than for the absence of the lines of hydrogen.

The line at 527 is probably the iron line E; this was seen in the hotter comets, namely, Comet Wells and the Great Comet of 1882, so that there is no discordance with regard to the appearance of this line. The other lines special to nebulæ are 479, 495, 509, and 554; but as no origins for these have yet been determined, it is not possible to explain their absence from cometary spectra. It is not improbable that 554 is an error in measurement for the manganese fluting at 558, the latter having been recorded by Mr. Taylor in the nebula of Orion.

[On November 25, Mr. Fowler attempted to compare this line, as seen in the planetary nebula G.C. 4373, with the manganese fluting, but the line was so faint with the 10-inch that no reliable comparison could be made. The line is certainly not far from the manganese fluting.—December 8.]

The apparent absence of the cool carbon flutings from nebulæ is in all probability due to insufficient observations, as indicated by the discussion of comets. The lowest temperature (magnesium) and the hot carbon stages of comets are both represented in nebulæ, and the

* O'Gyalla Observations, 1881, p. 5.

intermediate cool carbon stage is therefore not likely to be entirely absent.

The absence of the hot carbon fluting at 564 from the spectra of nebulæ may possibly be due to two causes. It is much fainter than either 517 or 468–474, and may have escaped notice on that account; or, as in the nebula in Andromeda, it may be masked in the same way as in comets.

It is suggested that the ordinary nebulæ are not hot enough to give the line or fluting at 568, but it appears when the swarms become more condensed, that is, in bright-line stars. The absence of 568 is therefore probably due to the low temperature of nebulæ.

II. Comparison of Comets and Auroræ.

If we exclude the exceptional cases of Comet Wells and the Great Comet of 1882, the number of lines and flutings recorded in comets is small, and therefore only the most general list of auroral lines must be taken for comparison. It would be unfair, for example, to take the long list of lines given by Gyllenskiöld. The lines stated are taken from the table which I gave in a note in January, 1888,* which has since been slightly rearranged before taking the means.

Comets.	Auroræ.	Probable origins.	λ of probable origins.
—	411	H	4101
[426]	426	?	?
431	431	CH	431
—	435	H	434
468—474	474—478	C (hot)	468—474
483	482	C (cool)	483
486	486	H	486
500	500	Mg	5006
517	517	C (hot)	517
519	519	C (cool)	519
521	522	Mg	521
—	531	?	—
—	535	Tl	535
—	539	Mn	540
546	545	Pb	546
558	558	Mn	558
561	—	C (cool)	561
564	—	C (hot)	564
568	—	Pb, Na	568
—	606	?	—
[615]	620	Fe	615
—	630	?	—

* 'Roy. Soc. Proc.,' vol. 43, p. 321.

Here, again, it will be seen, there are many striking coincidences. The hydrocarbon fluting at 431 and the hot and cool carbon flutings at 468—474, 483, 517, and 519 are common to both. The flutings of magnesium 500 and 521 and the flutings of lead and manganese at 546 and 558 are also common. The iron fluting at 615 is not seen in comets at ordinary temperatures, but since it was recorded in the Great Comet of 1882, it has been added, in brackets, to the list of cometary flutings. The line at 426, which was seen in Comet Wells, has also been added. It will be noted also that there are apparent discrepancies; some lines appearing only in comets and others only in auroræ. The explanation of the absence of hydrogen lines from comets which has already been given applies equally in this case. As there is no repulsion in the aurora similar to that exercised upon comets by the Sun, there is no reason for the absence of hydrogen. In the aurora the hydrogen lines may also be produced partly from aqueous vapour. The citron carbon flutings 561 and 564 have not been recorded in the aurora, although they are often seen in comets; their apparent absence from the aurora is probably because they fall in the brightest part of the continuous spectrum, and are consequently masked.

The lines special to auroræ are 531, 535, 539, 606, and 630.

III. Comparison between Comets and Bright-line Stars.

In the Bakerian Lecture for 1888 I gave a complete discussion of the spectra of bright-line stars, as far as the observations then went, and the conclusion arrived at was that they are nothing more than swarms of meteorites a little more condensed than those which we know as nebulæ. The main argument in favour of this conclusion was the presence of the bright fluting of carbon which extends from 468 to 474. This, standing out bright beyond their short continuous spectrum, gives rise to an apparent absorption-band in the blue. The varying measurements made by different observers may possibly have thrown a little doubt upon the conclusion that the bright band was due to carbon, but recent observations at Kensington have placed this beyond doubt. Direct comparisons of the spectrum of 2nd Cygnus with the flame of a spirit lamp were made by Mr. Fowler on September 22nd, and these showed an absolute coincidence of the bright band in the star with the blue band of carbon seen in the flame. The 10-inch equatorial and a spectroscope having one prism of 60° and two half-prisms were employed. On October 31st a similar comparison was made with 3rd Cygnus, and this also showed a perfect coincidence. It was found quite easy to get the narrow spectrum of the star superposed upon the broader spectrum of the flame, so that both could be observed simultaneously.

[On November 25, the spectrum of 1st Cygnus was observed by Mr. Fowler, and again he found a coincidence with the carbon fluting at 468—474. The observation was confirmed by Mr Baxandall.—December 9.]

Other evidence of carbon flutings was shown by slight rises in Vogel's light-curves near 517 and 564. These, however, could not be as well seen as the band in the blue, because they fall on the bright continuous spectrum from the meteorites. In the stars in Cygnus, Mr. Fowler detected brightenings near 517, and in 2nd and 3rd Cygnus, perfect coincidences were found with the fluting at 517 in the spirit-lamp flame. In this case both 517 and 468—474 were simultaneously seen to be coincident with flame-bands.

[In the observation of November 25, carbon 517 was also found in 1st Cygnus.—December 9.]

Measurements were made of the brightenings in the spectrum of γ-Cassiopeiæ by Mr. Fowler on September 18th, and these were also found to be coincident with the carbon flutings 517 and 468—474; the citron fluting at 564 was not seen. It may be remarked that C, F, and D_3 were seen very bright.

The conclusions drawn from my suggestions as to the presence of carbon, as well as hydrogen, in bright-line stars, are therefore strengthened.

In the following table, all the lines and flutings recorded in bright-line stars, with the exception of γ-Cassiopeiæ, are given. The lines recorded by Sherman in γ-Cassiopeiæ have not yet been confirmed.

Comets.	Bright-line stars.	Probable origins.	λ of probable origins.
—	4101	H	4101
431	—	CH	431
—	434	H	434
468—474	468—474	C (hot)	468—474
483	—	C (cool)	483
486	486	H	486
500	—	Mg	500
—	507	? Cd	508
517	517	C (hot)	517
519	—	C (cool)	519
521	—	Mg	521
[527]	527	Fe	527
—	540	Mn	540
546	—	Pb	546
558	558	Mn	558
561	—	C (cool)	561
564	564	C (hot)	564
568	568	Pb, Na	568
[579]	579	Fe	579
—	5872	? (D_3)	—
[589 (D)]	589	Na (D)	5889, 5895
—	635	?	—

The coincidences here are between the flutings of hot carbon, manganese 558, and Pb,Na 568. D has only been seen bright in one of the stars (γ-Argus), which is probably one of the hottest; since D was seen bright in two of the hottest comets, I have inserted it in the list of cometary lines and flutings, and [527] and [579] are added for the same reason.

Although nine lines or flutings are common to comets and bright-line stars, six occur in comets which do not appear in bright-line stars, and five in bright-line stars which do not appear in comets.

The apparent absence of hydrogen from comets has already been referred to, as well as the absence of D_3. The cool carbon flutings are not seen in the bright-line stars because the temperature is too high, and Mg 500 is absent for the same reason; Mg 521 is probably also absent because of the higher temperature. The lead fluting at 546 may be masked by continuous spectrum in the bright-line stars; at all events, it appears as an absorption-band when the swarms further condense. Besides the hydrogen and D_3 lines, the lines 507, 540, and 635 appear in bright-line stars, but not in comets.

IV. Comparison of Comets and Stars of the Mixed Fluting Group.

In the Bakerian Lecture I also gave evidence to show that stars of Group II (Vogel's Class IIIa) are of a cometary character, and a little more condensed than the bright-line stars. The grounds on which this conclusion was arrived at was the probable presence of bright carbon flutings, in addition to the metallic absorptions. Observations of α-Herculis and Mira Ceti by Mr. Fowler at Kensington and by myself at Westgate-on-Sea have fully confirmed this view. The rapid increase of brilliancy of the flutings of Mira at its maximum in 1888 left little doubt in my mind that they were due to carbon, and Mr. Fowler's comparisons showed perfect coincidences with the carbon flutings, with the dispersion of two prisms of 60°.

Some of the origins which I suggested for the dark bands have also been tested by direct comparisons. Dunér's bands 4 and 5 were found to be coincident with the manganese and lead flutings at 558 and 546 respectively, and band 3 was found to be coincident with the maganese fluting about 586.

[Mr. Maunder observed the spectrum of α-Orionis on December 16, 1887, and made comparisons with the spectra of carbon, sodium, and manganese, as given by a Bunsen flame. He states the results as

D 2

follows*:—"The carbon band at 5164 was coincident (within the limits of observation with this dispersion) with the bright space towards the blue of Band VI (Dunér's band 7), and the sodium lines were clearly represented by two dark lines near the middle of Band II (Dunér's band 3), but the two manganese bands observed, not only did not coincide with any great band of the spectrum, but were very far distant from any of them. There were, indeed, faint lines about the neighbourhood of either manganese band, but the entire spectrum is full of such lines, and no fluting, nor anything corresponding to one, could be detected near the place of these two bands. A third manganese band was very close to Band II (Dunér's band 3) of the stellar spectrum." On the other hand, Vogel measured the position of the sharp edge of a fluting in α-Orionis as 559·1, and Dunér's measures for the same vary from 557·5 to 559·3, none of which can be described as "very far distant" from the manganese fluting near 558. Mr. Maunder's observation can only be explained by assuming that the band in question is variable. This might be produced by variations in the intensity of the carbon flutings; the manganese fluting falls on the carbon fluting near 564, and, according to their relative intensities, the manganese fluting will be visible or will be marked by the carbon. According to Gore, the star was at a minimum in December, 1887.

The fluting near 586 corresponds to Dunér's band 2, for which Dunér measures wave-lengths varying from 585·4 to 586·1. It apparently escaped Mr. Maunder's notice, at the time he made his observations, that no reference was made in my paper of November, 1887, to any band in the star spectra which fell near the third fluting of manganese near 535. The first two flutings, near 558 and 586, fell so near to two of the dark bands in the spectra of the stars of Group II that there was strong ground for believing them to be due to manganese. This has since been abundantly confirmed by Mr. Fowler's direct comparisons of the manganese flutings with the spectra of several stars of the group.—December 9.]

Under the heading of "Dunér's Bands" I give the mean wave-lengths measured by Dunér for the dark bands, and the limits of the bright spaces which are due to carbon.

The figures first given refer to the sharp edges of the flutings; the other figures indicate approximately where the flutings fade away.

This comparison shows that there is a very close relation between comets and Group II independent of the probable origins suggested. Bright carbon flutings, the manganese fluting at 558, the lead fluting at 546, the iron fluting at 615, and the magnesium fluting 521 are common.

* 'Greenwich Observations,' 1887, p. 22.

Comets.	Dunér's bands.		Probable origins.	λ of probable origin.
—	461—451	Bright space ..	C_B	460—451
—	461—473	(10) Dark space ..	—	—
468—474	472—476	Bright space ..	C (hot)	468—474
—	476—486	(9) Dark space ..	—	—
483	—	—	C (cool)	483
—	495—486	? Bright fluting.	?	—
500	495—502	(8) Dark fluting ..	Mg	500
517	516—502	Bright fluting.	C (hot)	517
519	—	—	C (cool)	519
521	516—522	(7) Dark fluting ..	Mg	521
—	524—527	(6) Dark fluting ..	Ba (2)	526
546	544—551	(5) Dark fluting ..	Pb	546
558	559—564	(4) Dark fluting ..	Mn (1)	558
561	—	—	C (cool)	561
564	—	—	C (hot)	564
—	585—594	(3) Dark fluting ..	Mn (2)	586
[615]	616—630	(2) Dark fluting ..	Fe	615
—	647—668	(1) Dark fluting ..	?	—

As I showed in the Bakerian Lecture, Mg 500, when not masked by the broad carbon fluting at 517, is probably indicated by the dark band, for which Dunér gives the value 502 to 495, so that this may also be regarded as common to Group II stars and comets.

The cool carbon flutings are seen in comets, but not in stars of Group II, the reason being that the temperature is too great. The hot carbon fluting at 564 is in all probability present in stars of Group II, but is always masked, in some cases by continuous spectrum, and in others by the absorption fluting of manganese, which is nearly coincident with it.

The line, or probably fluting, at 495 has not yet been recorded in comets, but its association with the fluting at 500 in Nova Cygni indicates that its apparent absence is entirely due to incomplete observations.

The second fluting of manganese, near 586, though one of the most prominent in stars of Group II, has not been observed in cometary spectra, probably because there is not sufficient continuous spectrum from the sparse meteoritic background of the comet to produce the absorption of more than the first fluting of manganese.

Dunér's band 1, 647 to 668, has not yet had an origin assigned to it.

V. General Comparisons.

In the preceding tables I have shown that the spectra of nebulæ, auroræ, bright-line stars, and stars of Group II are closely related to the spectra of comets. In the table which follows all the spectra are

brought together and compared. It is not sufficient to show that each resembles comets in some respects, as each one might have some feature which was absent in the other. I, therefore, give the following table to show how far they resemble each other. In the last column the dark bands which are simply due to absence of radiation, and are not really absorption-bands, are omitted.

Nebulæ.	Aurora.	Comets.	Bright-line stars.	Stars with mixed flutings.
4101	411	—	4101	—
—	426	[426]	—	—
—	431	431	—	449 (bright space)
434	435	—	434	—
447	—	—	—	—
—	—	—	—	461—451 bright
468—474	474—478	468—474	468—474	472—476 „
479	482	483	—	—
486	486	486	486	—
4958	—	—	—	4958—486 bright
500	500	500	—	502—4959 dark
509	—	—	507	—
517	517	517	517	516—502 bright
—	519	519	—	—
520	522	521	—	522—516 dark
—	—	—	—	524—527 dark
527	—	[527]	527	—
—	531	—	—	—
—	535	—	—	—
—	539	—	540	—
546	545	546	—	544—551 dark
554	—	—	—	—
559	558	558	558	559—564 dark
—	—	561	—	—
—	—	564	564	—
—	—	568	568	—
—	—	[579]	579	—
—	—	—	—	585—594 dark
5872 (D_3)	—	—	5872	—
—	—	[589]	589	—
—	606	—	—	—
—	620	[615]	—	616—630 dark
—	630	—	635	—

It will be seen that there are three flutings which run through the five columns, namely, 468—474, 517, and 558; and four more, H 486, Mg 500, Mg 521, and Pb 546, occur in four out of the five columns. Out of the thirty-four lines or flutings given, there are nineteen which occur in less than three columns, but this number is greatly reduced when slight differences of temperature, masking effects, and the exceptional conditions of comets are taken into account.

It is now universally agreed that comets are swarms of meteorites,

and the tables which I have given show that nebulæ, bright-line stars, stars with mixed flutings, and the aurora have spectra closely resembling those of comets, and are therefore probably also meteoritic phenomena.

II. "The Presence of Bright Carbon Flutings in the Spectra of Celestial Bodies." By J. NORMAN LOCKYER, F.R.S. Received November 23, 1889.

One of the chief conclusions arrived at in my former papers was that not only the nebulæ but many of the so-called stars are really sparse groups of meteorites, the latter only differing from the former by the fact that they are more condensed. I also pointed out that if this conclusion were correct the spectra of both these classes of bodies should approximate to those of comets, in which carbon radiation is one of the chief features, while their meteoritic nature is generally accepted. Since those papers were written a further inquiry has been made, both by looking through the records of past observations, and by additional observations at Kensington and Westgate, with a view of gaining more information as to the presence or absence of bright carbon flutings in the spectra of nebulæ and stars.

Certain results have already been obtained which I think sufficiently interesting to communicate to the Society. Before these observations were made, I suggested that some of Vogel's observations might be interpreted to signify bright carbon, but there was then a little doubt as to the existence of the bright flutings in the stellar spectra, as their presence was only suggested in some cases by slight rises in the light curves.

The following is a list of the bodies which contain either one or both of the carbon flutings near 517 and 468—474, the latter being a group of flutings, which, as I have before shown,* sometimes has its point of maximum brightness shifted from 474 to 468. The fluting near 564 has been omitted from the table, as it is generally masked, either by continuous spectrum or by the superposition of the fluting of manganese near 558. The wave-lengths given are as measured by the various observers stated.

The spectrum of the aurora is added for the sake of completeness.

It will be seen from the table that the record of the presence of carbon is unbroken from a planetary nebula through stars with bright lines to those resembling α Herculis, *i.e.*, entirely through Groups I and II of my classification.

* 'Roy. Soc. Proc.,' vol. 35, p. 167.

Name.	Fluting 468—474.	Fluting 517.	Reference.
Planetary nebula	469·4 (Copeland)	.. ..	'Copernicus,' vol. 1, p. 2.
Nebula in Orion	470 (Taylor)	.. ..	'Monthly Notices,' vol. 49, p. 126.
Nebula, Gen. Cat., No. 4373	.. ..	518 (Vogel)	'Bothk. Beob.,' Leipzig, Heft 1, 1872, p. 57.
,, ,, ,, 4234	.. ..	518 (Vogel)	,, ,, ,, ,, ,,
,, ,, ,, 4390	.. ..	518 (Vogel)	,, ,, ,, ,, p. 58.
Nebula in Andromeda	468—474 (Fowler)	517 (Fowler)	'Roy. Soc. Proc.,' vol. 45, p. 216.
,, ,,	.. ..	517 (Taylor)	'Monthly Notices,' vol. 49, 126.
γ-Argus	468 (Ellery)	.. ..	'Observatory,' vol. 2, p. 418.
,,	464·6 (Copeland)	.. ..	'Copernicus,' vol. 3, p. 205.
Arg.-Oeltzen, 17681	461—470 (Vogel)	.. ..	'Astro-Phys. Obs. zu Potsdam,' vol. 4, No. 14, p. 16.
,, ,,	473 (Pickering)	.. ..	'Astr. Nachr.,' No. 2376.
Lalande, 13412	469 (Vogel)	.. ..	'Astro-Phys. Obs. zu Potsdam,' vol. 4, No. 14, p. 17.
1st Cygnus	470 (Wolf and Rayet)	.. ..	'Comptes Rendus,' vol. 65, p. 292.
,,	465—470 (Vogel)	.. ..	'Astro-Phys. Obs. zu Potsdam,' vol. 4, No. 14, p. 17.
,,	468—474 (Fowler)	517 (Fowler)	New observations.
2nd Cygnus	470 (Wolf and Rayet)	.. ..	'Comptes Rendus,' vol. 65 (1867), p. 292.
,,	464 (Vogel) middle of band.	.. ..	'Astro-Phys. Obs. zu Potsdam,' vol. 4, No. 14, p. 17.
,,	468—474 (Fowler)	517 (Fowler)	New observations.
3rd Cygnus	470 (Wolf and Rayet)	.. ..	'Comptes Rendus,' vol. 65 (1867), p. 292.
,,	461—468 (Vogel)	517 (Vogel)	'Astro-Phys. Obs. zu Potsdam,' vol. 4, No. 14, p. 292.
,,	468—474 (Fowler)	517 (Fowler)	New observations.
γ-Cassiopeiæ	.. ..	517 (Sherman)	'Astr. Nachr.,' No. 2691.
,,	468—474 (Fowler)	517 (Fowler)	New observations.
ο-Ceti	468—474 (Fowler)	517 (Lockyer and Fowler)	New observations.
α-Herculis	468—474 (Fowler)	517 (Lockyer and Fowler)	New observations.
α-Orionis	.. ..	517 (Lockyer and Fowler)	New observations.
Aurora	474—478 (Vogel)	.. ..	'Bothk. Beob.,' Leipzig, Heft 1, 1872, p. 43.
,,	.. ..	517 (Backhouse)	'Nature,' vol. 7, p. 463.

I should add that Mr. Fowler has glimpsed a line less refrangible than that at 500 in the spectrum of the ring nebula in Lyra. If this should turn out to be the carbon fluting at 517, it would seem that in that nebula we may have a state of condensation between those represented by the nebulæ of Orion and Andromeda, the carbon replacing the λ 500 fluting of magnesium in the nebulæ, as apparently happens in comets on their approach to perihelion.

III. "Some Observations on the Amount of Luminous and Non-luminous Radiation emitted by a Gas Flame." By Sir JOHN CONROY, Bart., M.A., Bedford Lecturer of Balliol College and Millard Lecturer of Trinity College, Oxford. Communicated by A. G. VERNON HARCOURT, LL.D., F.R.S. Received November 11, 1889.

[See page 55.]

IV. "On the Effects of Pressure on the Magnetisation of Cobalt." By C. CHREE, M.A., Fellow of King's College, Cambridge. Communicated by Prof. J. J. THOMSON, F.R.S. Received November 22, 1889.

(Abstract.)

It has long been known, from the classic researches of Dr. Joule, that a rod of iron free from stress increases in length when magnetised in a comparatively weak field. When, however, the strength of the field is continually raised, it has been found by Mr. Shelford Bidwell that the rod ceases to increase in length, and then shortens, so that in a sufficiently strong field the length becomes less than it was originally. It has also been found by Villari, Sir W. Thomson, and others that when a rod of iron is exposed to successive loadings and unloadings of a given weight in a magnetic field, there appears a corresponding cyclic change of magnetisation. In this cyclic change the maximum magnetisation occurs when the load is "on," or when the load is "off," according as the field is weaker or stronger than a certain critical field depending on the load, called by Sir W. Thomson the Villari critical field.

Cobalt has been found by Mr. Shelford Bidwell to shorten when magnetised in weak fields, but to lengthen in very strong fields. The field in which it ceases to shorten is very much higher than the field in which iron ceases to lengthen. Also in weak fields Sir W. Thomson has found the magnetisation of a cobalt rod under cyclic applications of tension to be least when the tension is "on."

Now Professor J. J. Thomson has shown that on dynamical principles the effect of changes of magnetisation on the length of a rod of magnetic metal, and the effect of changes in the length of the rod on the magnetisation, must be fundamentally connected. In his "Applications of Dynamics to Physics and Chemistry," he has arrived at mathematical equations connecting the two phenomena, such that from a knowledge of the one set of phenomena the character of the other set can be deduced.

The conclusions derived from the theory are in the case of iron in accordance with the results of experiment, at least in their general character. In cobalt there is also an agreement between theory and experiment, so far as Sir W. Thomson's experiments go. In the absence of further experiments it would, however, be impossible to tell whether or not this agreement extended to the strong fields in which occurred the important phenomena observed by Mr. Shelford Bidwell. The application of Professor J. J. Thomson's formulæ to Mr. Shelford Bidwell's results led him to the conclusion that under cyclic applications of pressure a cobalt rod should experience cyclic change of magnetisation, and that the maximum magnetisation should answer to pressure "on," or to pressure "off," according as the magnetic field was weaker or stronger than a critical field, corresponding to the Villari field in iron. It was for the purpose of determining whether such a critical field did actually exist that the present investigation was commenced at Professor J. J. Thomson's suggestion.

Employing the magnetometric method, it was found that the agreement between theory and experiment was at least as satisfactory in cobalt as in iron. The application of pressure cycles in a magnetic field led to a cyclic change of magnetisation in a cobalt rod, in which the maximum magnetisation occurred when pressure was "on," or when it was "off," according as the strength of the field was below or above 120 C.G.S. units. This accordingly was the Villari critical field foreshadowed by theory.

Various phenomena which promised to throw light on the true character of the relations of strain and magnetisation having been noticed at an early period of the investigation, it was decided to make an attempt to isolate the phenomena, and examine them exhaustively.

In weak fields the first pressure applied after the introduction of the cobalt rod into the magnetising coil caused a large increase in the induced magnetisation. As the strength of the field was raised this change in the magnetisation attained a maximum, then diminishing vanished in a field considerably stronger than the Villari field for the cyclic effect, and in all stronger fields consisted in a diminution of magnetisation. The fields in which the cyclic effect of pressure and the effect of the first pressure were absolutely greatest occurred in the

neighbourhood of the "wende-punkt," or point where the coefficient of magnetic induction is a maximum. Relatively, however, to the intensity of the pre-existing induced magnetisation both the effects continually diminished in importance, as the strength of the field was raised from zero. In the weakest experimental field the first pressure increased the induced magnetisation by over 50 per cent., and fully 4 per cent. of the magnetisation took part in the cyclic change accompanying the pressure cycles. In some respects these results present a close resemblance to those observed by Professor Ewing in iron.

It was found that the existence of pressure previous to and during the introduction of the rod into a coil traversed by a current had an effect of the same general character, though not exactly of the same magnitude, as the first application of pressure when the rod on its introduction into the coil was free from pressure. Also on the break of a current during which pressure cycles had been applied, the rod manifested a polar character, in that, when exposed a second time without intermediate demagnetisation to the same strength of current, it possessed a greater intensity of magnetisation when the current passed in one direction than when it passed in the other. Both these effects had critical fields where they vanished and changed sign, and these fields were close to, if not identical with, the field in which the effect of the first pressure vanished. In fields below the critical the magnetisation of the rod when exposed a second time, without intermediate demagnetisation, to a current of the same strength as one in which pressure cycles had just been applied was greatest when the direction of the current was unchanged; but in fields above the critical the reverse was the case.

Both Villari and Professor Ewing observed that after the break of the magnetising current cyclic changes of tension produced eventually in iron wires cyclic changes of the residual magnetisation. In these the maximum magnetisation answered, as in the induced magnetisation in fields below the Villari point, to tension "on." Professor Ewing apparently examined the effect only in weak fields, but he does not seem to have anticipated that it would change its character in stronger fields.

In the present investigation the existence of a cyclic change in the residual magnetisation of cobalt accompanying cyclic changes of pressure has been established, and the magnitude of the effect examined in a large number of fields, extending from 0 to 400 C.G.S. units. It was found that not only the magnitude but the sign even of the effect depended largely on the condition of the rod during the break of the current. When the rod was under pressure during the break, the residual magnetisation in the cyclic state showed a maximum under pressure, whatever was the strength of the pre-existing field. When, however, the rod was free from pressure during

the break of the current, it was only in the residual magnetisation left after the weakest fields that the maximum answered to pressure "on." When the strength of the pre-existing field was raised, the effect passed through the value zero and changed sign. In the absence of all pressure during the flow of the current this critical field was only about 18 C.G.S. units. The application of pressure cycles during the flow of the current raised the critical field to about 30 C.G.S. units, but in fields over 60 C.G.S. units seemed to have extremely little effect. The intensity of the residual magnetisation when the cyclic effect of pressure vanished was of course in either case only a small fraction of the intensity of the induced magnetisation in the Villari critical field.

In the absence of all pressure the residual magnetisation left after the break of weak fields was very small, but in the weakest experimental fields its amount was increased in the ratio of 4 or 5 to 1 by the application of pressure cycles during the flow of the current. In fields over 30 C.G.S. units this effect of pressure cycles became extremely small.

The application of the first pressure subsequent to the break of the magnetising current and the removal of a pressure existing during the break of the current were found to shake out a considerable amount of the residual magnetisation. The percentage shaken out by the application of the pressure was decidedly the higher in strong fields, but in weak fields the reverse was the case when pressure cycles had been applied during the flow of the current.

Various other kindred phenomena were observed, and the laws of their variation examined. Most of the results obtained are given in tables, and curves are drawn showing the dependence of the more important effects on the strength of the field.

All the observations were made on a single specimen of cobalt, and by repeating certain of the observations at intervals it was proved that the condition of the specimen must have remained essentially unaltered while the different phenomena were being examined. Thus the several effects can be directly compared, and their mutual relationships are not masked by those differences of condition which are sure to exist between different specimens. An attempt has thus been made from an analysis of the phenomena to attain some knowledge of the true character of the magnetic changes effected by the application of pressure.

The experiments were all conducted in the Cavendish Laboratory, and the author is much indebted to Professor Thomson for the facilities afforded him, and for suggestions as to the form of the apparatus and the methods employed.

V. "On the Steam Calorimeter." By J. JOLY, M.A. Communicated by G. F. FITZGERALD, F.R.S., F.T.C.D. Received November 26, 1889.

(Abstract.)

The theory of the method of condensation has been previously given by the author in the 'Proceedings of the Royal Society,' vol. 41, p. 352.

Since the publication of that paper a much more extended knowledge of the capabilities of the method has been acquired, which has led to the construction of new forms of the apparatus, simple in construction and easily applied. Two of these are described and illustrated, one of which is new in principle, being a differential form of the calorimeter. The accuracy of observation attained by this latter form is so considerable that it has been found possible to estimate directly the specific heats of the gases at constant volume to a close degree of accuracy.

An error incidental to the use of the method arising from the radiation of the substance, when surrounded by steam, to the walls of the calorimeter, is inquired into. It is shown that this affects the accuracy of the result to a very small degree, and is capable of easy estimation and elimination.

Further confirmation of the truth of the method is afforded in a comparison of experiments made in different forms of the steam calorimeter.

Various tables of constants are given to facilitate the use of the method, and the results of experiments on the density of saturated steam at atmospheric pressures, made directly in the calorimeter, are included. These are concordant with the deductions of Zeuner, based on Regnault's observations on the properties of steam, and were undertaken in the hope of affording reliable data on which to calculate the displacement effect on the apparent weight of the substance transferred from air to steam.

The communication is intended to provide a full account of the mode of application of the steam calorimeter.

VI. "On the Extension and Flexure of Cylindrical and Spherical Thin Elastic Shells." By A. B. BASSET, M.A., F.R.S. Received December 9, 1889.

(Abstract.)

The usual theory of thin elastic shells is based upon the hypothesis that the three stresses R, S, T, may be treated as zero, where R is the

normal traction perpendicular to the middle surface, and S and T are the two shearing stresses which tend to produce rotation about two lines of curvature of the middle surface. This hypothesis requires that these stresses should be at least of the order of the square of the thickness of the shell, for when this is the case they give rise to terms in the expression for the potential energy due to strain, which are proportional to the fifth power of the thickness, and which may be neglected, since it is usually unnecessary to retain powers of the thickness higher than the cube. It can be proved directly from the general equations of motion of an elastic solid, that this proposition is true in the case of a plane plate, provided the surfaces of the plate are not subjected to any pressures or tangential stresses, but there does not appear to be any simple method of establishing a similar proposition in the case of curved shells. I have therefore adopted this proposition as a fundamental hypothesis, and have endeavoured to establish its truth and to obtain a satisfactory theory of cylindrical and spherical shells in the following manner:—

Taking the case of a cylindrical shell, let OADB be a curvilinear rectangle described on the middle surface, of which the sides OA, BD are generators, and the sides AD, OB are circular sections. The resultant stresses per unit of length across the section AD consist of (1) a tension, T_1; (2) a tangential shearing stress, M_2; (3) a normal shearing stress, N_2; (4) a flexural couple, G_2, about AD; (5) a torsional couple H_1, perpendicular to AD; and the stresses across BD may be derived by interchanging the suffixes 1 and 2. Resolving along OA, OB, and the normal, and taking moments about these lines, we obtain the following equations,* viz.:—

$$\left.\begin{aligned}
\frac{dT_1}{dz}+\frac{1}{a}\frac{dM_1}{d\phi} &= X \\
\frac{1}{a}\frac{dT_2}{d\phi}+\frac{N_1}{a}+\frac{dM_2}{dz} &= Y \\
\frac{dN_2}{dz}+\frac{1}{a}\frac{dN_1}{d\phi}-\frac{T_2}{a} &= Z \\
\frac{1}{a}\frac{dG_1}{d\phi}+\frac{dH_1}{dz}+N_1 &= L \\
\frac{dG_2}{dz}+\frac{1}{a}\frac{dH_2}{d\phi}-N_2 &= M \\
(M_2-M_1)\,a-H_2 &= 0
\end{aligned}\right\} \quad\text{.................} \quad (1),$$

* Compare Besant, "On the Equilibrium of a Bent Lamina," 'Quart. Journ. Math.,' 1860.

where X,Y . . . denote certain expressions involving the bodily forces, such as gravity and the like, and the time variations of the displacements.

The values of the four couples may be calculated by a direct method, and are

$$\left.\begin{aligned} G_1 &= -\tfrac{4}{3}\,nh^3\mathfrak{F},\ G_2 = \tfrac{4}{3}\,nh^3(\mathfrak{E}+\mathfrak{A}/a) \\ H_1 &= -\tfrac{2}{3}\,nh^3(p+\varpi_3/a),\ H_2 = \tfrac{2}{3}\,nh^3p \end{aligned}\right\}\ldots\ldots\ldots \text{(ii)}.$$

To explain the symbols involved in these equations, let u, v, w be the displacements along OA, OB, and the normal; σ_1, σ_2, ϖ_3 the extensional and shearing strains along and about these directions respectively; then putting $E = (m-n)/(m+n)$, the symbols in (ii) are defined by the following equations:—

$$\left.\begin{aligned} \mathfrak{A} &= \sigma_1+E\,(\sigma_1+\sigma_2),\quad \mathfrak{B} = \sigma_2+E\,(\sigma_1+\sigma_2) \\ \mathfrak{E} &= \lambda+E\,(\lambda+\mu),\qquad \mathfrak{F} = \mu+E\,(\lambda+\mu) \\ \lambda &= -\frac{d^2w}{dz^2},\ \mu = -\frac{1}{a^2}\left(\frac{d^2w}{d\phi^2}+w\right)-\frac{E}{a}\,(\sigma_1+\sigma_2) \\ p &= -\frac{2}{a}\,\frac{d^2w}{dzd\phi}+\frac{1}{a}\,\frac{dv}{dz}-\frac{1}{a^2}\,\frac{du}{d\phi} \end{aligned}\right\}\ldots\ldots \text{(iii)}.$$

It is important to notice that the couples involve the extension of the middle surface as well as the change of curvature.

The expression for the potential energy is next found, and its value per unit of area of the middle surface is

$$\begin{aligned} W = {} & 2nh\{\sigma_1^2+\sigma_2^2+E\,(\sigma_1+\sigma_2)^2+\tfrac{1}{2}\,\varpi_3^2\} \\ & +\tfrac{2}{3}\,nh^3\{\lambda^2+\mu^2+E\,(\lambda+\mu)^2+\tfrac{1}{2}\,p^2\} \\ & +\tfrac{2}{3}\,nh^3(\mathfrak{A}\lambda'+\mathfrak{B}\mu'+\tfrac{1}{2}\,\varpi_3 p') \\ & +\frac{4nh^3}{3a}\,(\mathfrak{A}\lambda+\mathfrak{B}\mu+\tfrac{1}{2}\,\varpi_3 p)\ \ldots\ldots\ldots\ldots\ldots\ldots \text{(iv)}. \end{aligned}$$

The quantities λ', μ', p', depend partly upon quantities which define the bending, and partly upon the extension of the middle surface.

This expression is different from that obtained by Mr. Love, which arises from the fact that he has omitted to take into account several terms depending upon the product of the extensions and h^3. It will be noticed that (iv) reduces to the second line when the middle surface is inextensible, and in this case agrees with the expression obtained by Lord Rayleigh.*

* 'Roy. Soc. Proc.,' Dec., 1888.

The variational equation of motion may be written

$$\delta W + \delta \mathfrak{T} = \delta U + \delta \mathfrak{X} \ldots\ldots\ldots\ldots\ldots\ldots \text{(v)}$$

where $\delta\mathfrak{T}$ is the term depending on the time variations of the displacements, δU is the work done by the bodily forces, and $\delta\mathfrak{X}$ is the work done upon the edges of the portion of the shell considered, by the stresses arising from the action of contiguous portions of the shell.

Applying (v) to a curvilinear rectangle bounded by four lines of curvature, and working out the variation in the usual way, the line integral part will determine the values of the edge stresses $T_1, T_2 \ldots$, in terms of the displacements, and ought also to reproduce the values of the couples which we have already obtained; and the surface integral part will give the three equations of motion in terms of the displacements. These results furnish a test of the correctness of the work, and also of the fundamental hypothesis upon which the theory is based; for if we substitute the values of the edge stresses in terms of the displacements in the first three of (i), we ought to reproduce the equations of motion which we have obtained by means of the variational equation; and this is found to be the case.

The boundary conditions can be obtained by Stokes' theorem, which enables us to prove that it is possible to apply a certain distribution of stress to the edge of a thin shell, without producing any alteration in the potential energy due to strain.

The general equations, owing to their exceedingly complicated character, do not, except in special cases, readily lend themselves to the solution of mathematical problems; but, for the purpose of throwing some light upon the question raised by Mr. Love, as to the impossibility of satisfying the boundary conditions at a free edge, when a curved shell is vibrating in such a manner that its middle surface experiences no extension nor contraction throughout the motion, I have considered the following statical problem:—

A heavy cylindrical shell, whose cross section is a semi-circle, is suspended by vertical bands attached to its straight edges, so that its axis is horizontal, and is deformed by its own weight; required the strain produced.

We shall assume that the displacement at every point of the middle surface lies in a plane perpendicular to the axis, and we shall suppose that the necessary stresses are applied to the circular edges.

Measuring ϕ from the lowest point and putting R for the change of curvature along a circular section, we find that

$$\frac{R}{\sigma_2} = \frac{E}{a} + \frac{3a(\frac{1}{2}\pi - \phi\sin\phi - \cos\phi)}{h^2\{\frac{1}{2}\pi - \cos\phi + \frac{3}{2}E(\frac{1}{2}\pi - \phi\sin\phi - \cos\phi)\}} \ldots\text{(vi)}.$$

Since h is small compared with a, this equation shows that the

change of curvature is large in comparison with the extension, except at points in the neighbourhood of the edge, where $a\left(\frac{1}{2}\pi-\phi\right)$ is comparable with h.

It is also shown that the tension T_1 parallel to the axis, and the couple G_2 about a circular section do not vanish at the circular edges, but have finite values; and therefore a tension and a couple of the proper amount, which tends to produce synclastic curvature of the generating lines must be applied at the circular edges. If, therefore, this force and couple were removed, anticlastic curvature of the generating lines would be produced, and this would involve extension of the middle surface parallel to the axis. It is, however, obvious that a thin shell, under these circumstances, does not assume a saddle-back form, and therefore the anticlastic curvature, and the extension upon which it depends, must be exceedingly small, except in the neighbourhood of the circular edges.

The difficulty of satisfying the boundary conditions at a curved free edge, when the middle surface is supposed to be inextensible, partly arises from the fact that it is impossible for the flexural couple about the curved edge to vanish, unless some extension or contraction takes place in the neighbourhood of the edge; but the inference to be drawn from the statical problem considered above is, that when a thin shell, whose edges are free, is vibrating, the amplitudes of those terms upon which the extension depends are small in comparison with the amplitudes of those terms upon which the bending depends. Moreover, a variety of results which have been obtained during recent years indicate, that the pitch of notes which depend upon extension is very high, compared with the pitch of notes which depend upon flexure; and this circumstance, combined with the smallness of the amplitudes of the extensional vibrations, points to the conclusion that the former notes are usually feeble in comparison with the latter.

The values of the edge stresses and the equations of motion are also obtained for a spherical shell, but the work is the same as in the case of a cylindrical shell, except in matters of detail.

The Society adjourned over the Christmas Recess to Thursday, January 9th, 1890.

Presents, December 19, 1889.

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"Some Observations on the Amount of Luminous and Non-luminous Radiation emitted by a Gas Flame." By Sir JOHN CONROY, Bart., M.A., Bedford Lecturer of Balliol College and Millard Lecturer of Trinity College, Oxford. Communicated by A. G. VERNON HARCOURT, LL.D., F.R.S. Received November 11. Read December 19, 1889.

In 1863 Julius Thomsen communicated to the "Naturforscher-versammlung" at Stockholm an account of some determinations he had made of "The Mechanical Equivalent of Light," and an abstract of this paper appeared in 'Poggendorff's Annalen' (vol. 125, 1865, p. 348).

He allowed the radiation from a sperm candle, a moderator lamp, and a gas flame, to fall on the face of a thermopile, and noted the deflection of the needle of a galvanometer in the thermoelectric circuit, when the radiation fell directly on the pile, and when it did so after passing through 20 cm. of water contained in a glass cell. In order to reduce the readings to absolute measure, he placed a glass globe containing hot water in front of the thermopile, and observed the deflection of the galvanometer; the mass of water contained in the globe, the water-equivalent of the globe, the temperature of the water, and of the room, being all known, from the observed rate of cooling the radiation, calculated by Dulong's formula, was determined.

By assuming that the 20 cm. of water absorbed all, or nearly all, the heat rays (*i.e.*, the non-visible), and transmitted nearly all the light rays, the loss of light being found experimentally to be only 13 per cent., he was able to calculate the proportion of light and heat (*i.e.*, visible and non-visible) radiations emitted by the flames, and to determine the mechanical equivalent of the light. He found that the ratio of the luminous to the total radiation was about the same for the candle, the oil lamp, and the gas flame, the amount being nearly 2 per cent.

He states "a flame whose light intensity is equal to one light unit, that is, one due to the combustion of 8·2 grams of spermaceti per hour, radiates as light per minute an amount of heat which would raise the temperature of 4·1 grams of water one degree." On this last sentence it must be remarked that a light unit cannot be expressed by a statement of the mass of a combustible consumed, but perhaps the original paper contains qualifications omitted in the Abstract.

Melloni (' Comptes Rendus,' vol. 31, 1850, p. 476) states "Le rayonnement de la flamme d'huile contient 90 parties sur 100 de cette espèce de chaleur (*i.e.*, chaleur obscure), le rayonnement du platine

incandescent en a 98 pour 100, et celui de la flamme d'alcool 99 pour 100."

Tyndall ('Phil. Mag.,' 1864, vol. 28, p. 335; and 'Contributions to Molecular Physics,' p. 260) states that he placed in front of his thermopile a rock-salt cell and observed the deflection of the galvanometer when the radiation from (1) an incandescent platinum wire, (2) the brightest part of a gas flame, (3) an electric arc lamp, passed through the cell, filled first with pure bisulphide of carbon, and then with bisulphide of carbon in which iodine had been dissolved; he found that the percentages of luminous to total radiation from the three sources were—

Incandescent platinum..............	4·17
Bright part of gas flame............	4·0
Arc lamp..........................	10·0

From measurements made by Mr. Merritt and Mr. Nakano ("Efficiency of Methods of Artificial Illumination," by Professor E. Nichols; 'The Electrical Engineer,' May, 1889), the efficiency (*i.e.*, the ratio of luminous radiation to total radiation) of arc and incandescent electric lamps appears to be about 10 and 5 per cent. respectively.

The experiments of which an account is contained in this paper were commenced with the object of repeating and extending Thomsen's observations; soon after they had been begun a notice appeared in 'Nature' (July 4, 1889) of a communication made on June 7, 1889, to the Physical Society of Berlin, by Dr. R. von Helmholtz, on the radiation from flames, and under these circumstances it seemed desirable to finish the experiments which had been already commenced, but not to go on with the enquiry until Dr. von Helmholtz's paper should have been published in full.

The experiments were made by allowing the radiation from an Argand gas flame to fall on the face of a thermopile, either directly or after passing through different thicknesses of water, or of a solution of alum in water, contained in cells with glass ends.

Instead of keeping the source of heat at a fixed distance from the thermopile, and noting the deflections of the galvanometer, which was the method employed by Melloni and those who have followed him, the distance between the source and the face of the pile was made the variable, and the distances noted at which equal deflections were produced; thus the necessity for calibrating the galvanometer was avoided, and also for assuming that the (corrected) deflections of the needle were strictly proportional to the amount of radiant energy incident upon the face of the pile.

On the other hand, it was necessary to assume that the amount of energy which reached the thermopile varied inversely as the square

of the distance from the source, and as the flame and chimney were of considerable size, the position of the source was not well defined; it was also necessary to assume that the amount of absorption due to the air, or to the aqueous vapour it contained, was negligible, or at least that it could be allowed for.

An ordinary thermopile with fifty-four couples and a low resistance Nobili's galvanometer were used. The galvanometer needles, which were rendered as nearly astatic as possible, were suspended in the ordinary way by a silk fibre, a plane mirror was attached to them, and the image of a slit placed in the slide-holder of a magic lantern thrown by reflection from the mirror on to a scale placed about 0·8 metre from the galvanometer. The scale was divided into half centimetres, and the position of the line of light was read to 1 mm. by estimation. A deflection of 4° on the circle of the galvanometer corresponded to about five divisions on the scale.

In order that the galvanometer should have a fairly fixed zero, it was found necessary to place a weak magnet, a slightly magnetised knitting needle, at a short distance from it; this reduced the sensibility of the instrument, but it was still much more sensitive to the feeble thermoelectric currents than a low resistance (0·5 ohm) Thomson galvanometer of the ordinary pattern.

The galvanometer coil was nearly perpendicular to the magnetic meridian, and the adjusting magnet so placed that the needles were parallel to the axis of the coil.

The thermopile was fixed at the end of a horizontal stand, to which a divided scale was attached, and was connected by covered copper wires with the galvanometer, a three-way plug being inserted in the circuit.

Both faces of the thermopile, which had been carefully blackened with camphor smoke, were exposed; the radiation from the lamp whose light-giving power was to be measured fell on the one, and the radiation from a compensating source of heat, an Argand with a metal chimney, on the other. Tin-plate screens were placed on either side of the pile, and the space between them stuffed with cotton wool, to screen it from air currents and from the radiation of surrounding objects, it being impossible to use the reflecting cones with the method of measurement employed.

Four cells of wood well soaked in paraffin were used to hold the water and alum solution; their ends were closed with pieces of crown glass, cut from the same plate of Messrs. Chance's manufacture, which were pressed against the ends of the cells by wooden pressure-plates and screws, a washer of vulcanised sheet-india-rubber being interposed between the cell and the glass. This arrangement enabled the same pair of plates to be used with all four cells. The plates were 1·5 mm. thick, the cells were 15 cm. deep, 10 cm. wide, and

1 cm., 5 cm., 10 cm., and 15 cm., long; each of the washers was about 1 mm. thick, so that the radiations which passed through the cells had to traverse 3 mm. of glass and 1·2 cm., 5·2 cm., 10·2 cm., or 15·2 cm. of water in Cells I, II, III, and IV.

The determinations were made by placing one of the cells in front of the thermopile, and about 9 cm. from it, a screen with a rectangular aperture, 5 cm. by 3 cm., being fixed at about 2 cm. from the end of the cell nearest the pile, in order to prevent any radiations reflected from the surfaces of the water in the cell reaching the pile; the glass plates were always well cleaned immediately before being used, and the cell filled with freshly filtered distilled water.

An Argand burner, 1·5 cm. in diameter, with a glass chimney 4·5 cm. in diameter and 15 cm. high, was placed beyond the cell, a tin-plate screen being interposed.

In the experiments recorded in this paper the axis of the burner was 31·8 cm. from the face of the pile.

The index of the galvanometer was then, if necessary, brought to zero and the circuit closed; closing the circuit almost always caused the line of light to move in one or other direction. By adjusting the height of the flame of the compensating burner, which was invariably small, from 0·3 cm. to 0·8 cm., and its distance from the thermopile, the line of light was brought back to the zero, or nearly so.

A horizontal wire was clamped to the stand of the burner whose radiation was to be observed, 10 cm. above the plate of the burner, and by means of a tap the height of the flame so regulated that its tip just appeared above the wire.

The metal screen was then removed, the amplitude of the first swing of the galvanometer needle observed, and the screen replaced.

The oscillations of the needle were extremely slow, and, therefore, instead of always waiting till it had completely come to rest again, as soon as the oscillations had become small, about half a division of the scale or less, the screen was again removed and another reading made, care being taken to always remove the screen whilst the index was passing the point taken as zero, and in the opposite direction to that in which the deflection due to the heat would occur.

In this way twelve readings were made, and then the cell was removed and the lamp placed at a greater distance from the pile, which was shielded by a tin-plate screen which could be moved to and fro by strings.

It was usually necessary to readjust the compensating lamp, and when the index had again been brought to zero the screen was removed and the deflection noted. After a few trials a position was found for the lamp in which the deflection was about the same as that produced by the radiation which had passed through the water; a reading of the deflection was made with the lamp in this position, and

then it was moved 10 cm. along the scale, and another readiug made; the distances were thus ascertained at which the whole radiation from the lamp produced a somewhat less and a somewhat greater effect upon the galvanometer than had been produced by that portion of the radiation which had passed through the glass and water of the cell, with the lamp close to the pile.

The measurements were repeated six times for each position of the lamp; a curve was then plotted with the distances at which the lamp had been placed when no cell had been interposed between it and the pile as abscissæ, and the mean deflections corresponding to each of these positions as ordinates, and then the abscissa of a point whose ordinate was equal to the mean deflection when the cell had been interposed ascertained. This indirect method was adopted, as it would have been impossible, without making a very large number of measurements, to have ascertained the exact position of the lamp for which the direct radiation produced the same effect as that portion of it which had passed through the water.

Table I gives one set of readings made in this way with Cell I, and Table II the mean of three sets of readings made with the four cells, and with the lamp at different distances from the thermopile, the actual readings being about as concordant as those contained in Table I.

Table I.

Deflection of Galvanometer.

							Mean.
With cell.........	3·5 3·7	3·6 3·8	3·8 3·9	3·7 3·6	3·6 3·8	3·7 3·6	3·7
Lamp at 110......	4·5	4·2	4·1	4·0	4·3	4·0	4·2
„ 120......	4·2	3·6	3·3	3·2	3·6	3·6	3·6
„ 130......	3·5	3·4	3·0	3·2	3·0	2·8	3·15

Table II.

Mean Deflection of Galvanometer.

				Mean.
Cell I	3·7	4·0	4·25	3·98
Cell II................	2·05	2·27	2·2	2·17
Cell III	1·8	1·9	1·8	1·83
Cell IV................	1·9	1·9	1·9	1·9
Lamp at 110...........	4·2	4·13	4·1	4·14
„ 120	3·6	3·7	3·6	3·63
„ 130...........	3·15	—	—	—
„ 160...........	2·4	2·43	2·2	2·34
„ 170...........	2·0	2·03	2·15	2·06
„ 180...........	1·8	1·8	1·9	1·83
„ 190...........	1·7	1·76	1·7	1·72

In the case of the first set of measurements made with Cell I (those contained in Table I) the equivalent distance for the lamp is

clearly at about 120 on the scale, and its value, as deduced in the manner above described, is 118·5. From the face of the thermopile to the zero of the scale was 40 cm.; this quantity had, of course, to be added to the scale readings of the position of the lamp. The distance of the axis of the lamp from the face of the thermopile, when the cell was interposed, was 31·8 cm.; the glass and water of the cell being more refractive than air, interposing the cell virtually reduced the distance between the pile and the lamp. The rays which traversed the cell differing in refrangibility, and being incident upon the glass at different angles, the simplest form of the formula $x = e\left(1 - \frac{1}{n}\right)$ (in which n was taken as 1·33, and the glass and water treated as if they had the same index) was used for calculating the optical shortening of the distance.

The values of x for the four cells were 0·4 mm., 1·4 mm., 2·7 mm., and 4 mm.; these amounts subtracted from 31·8 cm., gave the virtual distances in the four cases.

Calling the two distances of the lamp d_1 and d_2, the intensity of the lamp I, and the coefficient of transmission K,

then $$\frac{\mathrm{I}}{d_1^2} = \mathrm{K}\frac{\mathrm{I}}{d_2^2}, \text{ whence } \mathrm{K} = \left(\frac{d_2}{d_1}\right)^2.$$

Table III gives the results of measurements made in this way with the different cells. Column 2 gives the values of d_1, that is, the scale reading of the position of the lamp without the cell, plus the distance between the zero and the pile; column 3 the corrected distances with the cell, *i.e.*, the measured distance less the correction for the refraction of the cell, and column 4 the coefficients of transmission.

Table III.

	d_1.	d_2.	K.
Cell I	154·0	31·4	0·04157
	148·0	31·4	0·04501
	158·5	31·4	0·03925
			0·04194
Cell II	204·0	30·4	0·02221
	200·0	30·4	0·02310
	208·5	30·4	0·02126
			0·02219
Cell III	215·0	29·1	0·01832
	225·0	29·1	0·01673
	224·0	29·1	0·01688
			0·01731

Cell IV................	215·0	27·8	0·01672
	220·0	27·8	0·01593
	215·0	27·8	0·01672
			0·01646

It is usually stated that water saturated with alum is more adiathermanous than pure water.

Melloni found ('Annales de Chimie,' vol. 153, 1833, p. 1) that 12 per cent. of the total radiation of an oil lamp with double air current passed through 9·21 mm. of a solution of alum contained in a glass cell, whilst 11 per cent. passed through the same thickness of water, or that the diathermancy of these two liquids in glass cells was practically the same.

It seems very unlikely that no other observations should have been made on this point, but a careful search has failed to disclose the record of any.

A solution of ammonia alum, saturated at about 15°, was prepared, and some preliminary observations were made with it in the manner already described; the coefficient of transmission for Cell I appeared to be slightly less, and that for Cell II slightly greater than when the cells were filled with water, but the differences were so small that it was thought that it would be more satisfactory to fill Cell I alternately with the alum solution and with pure water, and note the deflections of the galvanometer. Table IV gives a number of these readings: twelve were first made with the alum solution, and then twelve with water, and then twelve more with the alum, and finally twelve with water.

Table IV.

Deflection of Galvanometer.

							Mean.
Cell I. With solution of alum.....	3·7	3·8	3·2	3·7	3·2	3·5	3·55
	3·6	3·0	3·4	3·7	3·7	3·9	
	3·5	3·8	3·8	3·7	3·7	3·6	
	3·5	3·5	3·3	3·1	3·6	3·8	
Cell I. With water...............	3·4	3·6	3·3	3·9	3·6	3·4	3·60
	3·9	3·7	3·5	3·6	3·7	3·5	
	3·9	3·8	3·4	3·6	3·7	3·7	
	3·6	3·7	3·4	3·2	3·6	3·7	

The table shows that with the form of apparatus used there is no measurable difference between the absorption of an alum solution contained in a glass cell and that of pure water.

The amount of light transmitted by the four cells was determined, the photometric arrangement used being the one described in the 'Philosophical Transactions,' A, vol. 180, 1889, p. 248. It consisted essentially of a small Argand gas burner and of two mirrors, so placed that they reflected the light of the lamp towards each other. The photometer was placed between them, and the cell between the photometer and one of the mirrors, and six readings made of the position in which there was equality of illumination; the cell was then placed on the other side of the photometer, and six more readings made of the new position of equality of illumination; from these measurements the value of k, the coefficient of transparency, was calculated by the expression (*loc. cit.*, p. 250) $k = \dfrac{x_1(x - x_2)}{x_2(x - x_1)}$; x being the distance between the two images of the lamp, x_1 and x_2 the two positions of the photometer in which there was equality of illumination, the optical shortening of the path of the light due to its passage through the glass and water being, of course, allowed for.

Table V gives these results.

Table V.

	x_1.	$x - x_1$.	x_2.	$x - x_2$.	Per cent. of incident light transmitted.
Cell I........	185·5	199·1	199·3	185·3	86·62
	186·0	198·6	199·0	185·6	87·35
	185·5	199·1	199·1	185·5	86·80
				Mean..	86·92
Cell II.......	184·7	198·9	199·2	184·4	85·96
	184·8	198·8	199·1	184·5	86·14
	185·2	198·4	199·3	184·3	86·32
				Mean..	86·14
Cell III	182·9	199·4	198·1	184·2	85·29
	182·7	199·2	198·6	183·7	84·84
	182·7	199·2	198·6	183·7	85·11
				Mean..	85·08
Cell IV	182·3	198·7	198·6	182·4	84·26
	182·8	198·2	198·3	182·7	84·98
	182·9	198·1	198·6	182·4	84·80
				Mean..	84·68

The table shows that the loss of light increased from 13·1 per cent. with Cell I to 15·3 per cent. with Cell IV; the loss being due to

reflection at the surfaces of the glass and water, and to obstruction (*i.e.*, absorption and scattering) by the glass, and by the water. Calling r and r_1 the ratios of the light reflected from the surface of the glass and water and obstructed by the glass, at the two ends of the cell, to the light incident upon them, α the coefficient of transmission of the water, and t the thickness of the water, then the intensity of the transmitted beam is given by the expression $i = I\rho\rho'\alpha^t$, where $\rho = (1 - r)$ and $\rho' = (1 - r_1)$. I, i, and t being known, by eliminating $\rho\rho'$ α can be readily calculated.

Table VI gives the values of α for a thickness of 1 cm. of water, obtained by combining in pairs the values obtained with the four cells.

Table VI.

	Values of α.
	0·9977
	0·9974
	0·9981
	0·9975
	0·9983
	0·9990
Mean..	0·9980

In a former paper ('Phil. Trans.,' A, 180, 1889, p. 280) it is shown that the value of α per millimetre, for the crown and flint glass experimented with, was 0·99735 and 0·99884; the transparency of water appears to exceed considerably that of either kind of glass, being 0·9998 per millimetre.

From the mean value of α the values of ρ, on the assumption that $\rho = \rho'$, were obtained by calculating the value of α^t for the different thicknesses of water, and then introducing this value into the equation $i = I\rho\rho'\alpha^t$, where i is the measured amount of the percentage transmitted by each of the cells.

Table VII.

	Values of ρ.
Cell I....	0·9334
" II....	0·9330
" III....	0·9319
" IV....	0·9343
	0·9331

Hence the amount of light reflected at the two surfaces of each of the glass plates, and obstructed by the glass, would appear to be about 6·69 per cent.

The agreement between the values of ρ, as deduced from the measurements made with each of the four cells, confirms the general accuracy of the photometric observations.

Glass and water transmit, as is well known, radiations differing in wave-length with very different degrees of facility; all the kinds of radiations which affect the eye as light suffer about the same amount of absorption (*i.e.*, these media are colourless); but, as Melloni showed many years ago, the case is very different when the total radiation from any source of light and heat is considered.

Table VIII gives the percentage amounts of total and visible radiation transmitted by the four cells, and also the transmission coefficients A and α for the total and visible radiations, as deduced from the measurements made with each pair of cells.

Table VIII.

	Per cent. total radiation transmitted.	Per cent. visible radiation transmitted.	A.	a.
Cell I. 3 mm. glass and 12 mm. water	4·194	86·92		
			0·8529	0·9977
Cell II. 3 mm. glass and 52 mm. water	2·219	86·14		
			0·9515	0·9975
Cell III. 3 mm. glass and 102 mm. water	1·731	85·08		
			0·9900	0·9990
Cell IV. 3 mm. glass and 152 mm. water	1·646	84·68		

The table shows that the percentage amount of visible radiation absorbed by the water increases regularly with the thickness, but that in the case of the total radiation each additional centimetre of water absorbs less than those that have preceded it, and that the transmission coefficients for the total radiation increase as the thickness increases, whilst those for the visible radiation remain nearly constant. From the values of these coefficients it appears that a thickness of 3 mm. of glass and 102 mm. of water is not sufficient to arrest all the non-luminous radiations emitted by an Argand gas burner. The transmission coefficients for the total and visible radiations as deduced from the measurements made with Cells III and IV are much closer together than those deduced from the measurements made with Cells I and II, and II and III, and this seems to show that the amount of non-luminous radiation which passed through Cell IV was very small, and that, therefore, the 1·646 per cent. transmitted consisted almost exclusively of visible radiation, *i.e.*, light.

The photometric measurements show that 84·68 per cent. of the

incident light traversed Cell IV; hence it would appear, if we assume that neither the air nor the aqueous vapour absorbed a measurable amount of the radiation, that the total radiation of the gas burner contained $1{\cdot}646 \times \frac{100}{84{\cdot}68}$ or 1·94 per cent. of luminous radiation, a result that agrees with that obtained by Julius Thomsen.

As has already been stated, the method employed was based on the assumption that the amount of absorption due to the air or to the aqueous vapour it contained was negligible, or at least that it could be allowed for. The experiments were made in an underground room in which the temperature and the hygrometric condition of the air varied but slightly. The readings of a wet and dry bulb thermometer never differed during the course of the experiments more than about 1° F., the temperature of the air varying from about 59° F. to 67° F. Hence it was always nearly saturated, and the mass of water-vapour per unit volume of air was nearly the same.

Professor Tyndall states ('Contributions to Molecular Physics,' p. 133) that 4 feet of saturated air (the temperature of the air and the nature of the source of radiation, which from the diagram was apparently a gas flame, are not mentioned) absorbed $5\frac{1}{2}$ per cent. of the total radiation.

If we assume that for very small angles (and in the course of these experiments the angles never exceeded 4°) the deflections of the galvanometer were strictly proportional to the amount of radiant energy incident upon the face of the thermopile, and that the radiation from the lamp suffered no absorption before it reached the thermopile, then the deflections of the galvanometer would vary inversely with the square of the distance of the lamp. Table II shows that the mean deflection when the lamp was 150 cm. from the thermopile was 4·14 scale divisions. The deflections corresponding to other positions of the lamp were calculated by the expression $\frac{(150)^2 \times 4{\cdot}14}{x^2} = d$, where x is the distance of the lamp, and d the scale reading; the results are set forth in Table IX, column 2.

If, however, a portion of the radiation from the lamp was absorbed by the air, or the aqueous vapour it contained, then the decrease in the deflection as the lamp was moved further and further off would be partially due to the increased amount of absorption produced by the longer column of air and aqueous vapour.

Taking Professor Tyndall's value for the absorption (5·5 per cent. in 4 feet), the percentage amount that would be absorbed in passing through 10 cm., 50 cm., 60 cm., 70 cm., and 80 cm. of saturated air was calculated, and thence, by the expression given above, the value for the deflection; x^2 being taken as $\frac{(\text{distance of lamp})^2 \times 100}{100 - \text{absorption}}$.

These values are contained in Table IX, column 3; they agree more closely with the observed values contained in column 4 than those calculated on the assumption that there was no absorption. The deflections of the galvanometer, however, were so small, and therefore the difference between the two sets of calculated values for the deflections, and the observed values, so slight, that no very definite conclusions can be drawn from them; they seem, however, to show that some absorption did take place, and to about the same amount as stated by Professor Tyndall.

Table IX.

Distances of lamp.	Deflection of galvanometer.		
	Calculated.		Observed.
cm.			
150	..	..	4·14
160	3·64	3·62	3·63
200	2·33	2·28	2·34
210	2·11	2·05	2·06
220	1·92	1·86	1·83
230	1·76	1·70	1·72

In the experiments made with Cell IV, the radiation from the lamp had to traverse about 15 cm. of air when the cell was interposed between the lamp and the pile, and about 215 cm. when the cell was removed. The absorption in the former case must, according to Professor Tyndall's experiments, have been insensible, and in the latter case have amounted to about 9·7 per cent.; assuming that the absorption is proportional to the length of air traversed, an assumption which in all probability is not strictly true.

Calling the total amount of radiation from the lamp, 100, the expression for K under the given conditions is $\frac{90 \cdot 3}{84 \cdot 68}\left(\frac{d_2}{d_1}\right)^2 = 1 \cdot 751$.

Thus, allowing for the absorption due to the aqueous vapour, and to the loss which the light suffered in passing through the cell, it appears that the total radiation from the lamp consisted of 1·75 per cent. luminous and 98·25 per cent. non-luminous radiation: a somewhat smaller value for the percentage of luminous radiation than that found by Julius Thomsen.

Conclusion.

These experiments show:—

(1.) That 3 mm. of glass and 10 cm. of water transmit a small

portion of the non-luminous radiation of an Argand gas burner, but that when the thickness of the water is increased to 15 cm. the transmitted radiation consists exclusively, or almost exclusively, of those kinds of radiation which affect the eye as light.

(2.) That with the form of apparatus employed (a thermopile and galvanometer) there is no measurable difference between the diathermancy of pure water and of a solution of alum.

(3.) That the radiation from an Argand gas burner consists of about 1·75 per cent. luminous and 98·25 per cent. non-luminous radiation.

[*Note.*—After this paper had been presented to the Royal Society, I was made aware for the first time, by means of a reprint in the December number of 'Wiedemann's Annalen' (vol. 38, 1889, p. 640), of a paper on "the mechanical equivalent of light," which O. Tumlirz had communicated to the Vienna Academy in the summer of this year. He states that the total radiation from the amyl acetate lamp he employed contained 2·4 per cent. of luminous radiation. He obtained this result by allowing either the whole radiation from the lamp, or that portion of it which had traversed a glass cell containing water, to fall upon the face of a thermopile, and noting in the two cases the deflections of the needle of a galvanometer in the thermo-electric circuit.—December 27, 1889.]

"Observations on the Spark Discharge." By J. JOLY, M.A., B.E. Communicated by Prof. G. F. FITZGERALD, F.R.S. Received June 15,—Read June 20, 1889.

[PLATES 1—5.]

Path of Discharge over the Surface of a Dielectric.

The subject of dust-figures produced by electrical discharge has received much attention at various times. In the following notes I have refrained, to the best of my knowledge, from going over old ground. The subject has been reviewed in Lehmann's recently published 'Molekularphysik.' It is a sufficient substitute for the customary *résumé* of past observations to refer to that work.

(1.) When a spark discharge occurs in a homogeneous dielectric medium, the path of discharge, as is known, in general lies in a fairly straight line between the points of discharge. If the dielectric medium be heterogeneous in character, the path chosen by the spark will vary with the circumstances. Thus, if the straight line between the conductors be interrupted by a layer of a substance offering a higher resistance to discharge than the surrounding dielectric, the

discharge may be chiefly confined to this surrounding dielectric, spreading over the surface of the layer in ramifying lines.

This ramifying discharge is depicted in some measure in Lichtenberg's figures and in Antolik's. But these lose some of their value in the fact that they represent an electrified condition of the surface of a dielectric obtaining subsequent to the period of discharge. The path of discharge revealed in the figures described in this paper is, on the other hand, laid down by the outspreading current in the act of discharge. There is some resemblance between all three patterns of figures, more especially with Antolik's, whose figures are obtained with a similar disposition of apparatus.

The following are the arrangements used in obtaining the dust-figures:—

A plate of glass is coated evenly with a thin layer of lycopodium powder. This is best done by placing the glass on the bottom of a deep box and shaking the powder from a linen bag, surrounded with a couple of folds of gauze, through a hole in the lid of the box. The plate is next transferred to the surface of a smooth sheet of metal. Wires from the + and − poles of a Ruhmkorff coil are then brought down just to meet the surface of the glass, touching it at points 6 or 8 cm. removed from each other and symmetrical about the centre of the plate. It is immaterial if the underlying conductor be connected to earth or not. By drawing back the hammer of the coil and again bringing it sharply forward, a single make and break is effected. At the moment of "make," hardly any disturbance of the dust, save for a couple of millimetres around the poles, is noticeable, but at the moment of "break" the dust is suddenly agitated and thrown into the pattern shown in Plate 1, a flash of burning lycopodium sometimes accompanying.

This figure depicts the case where the poles have been brought into such proximity as to permit, in addition to the ramifying discharge, the direct passage of a spark. If the poles on the glass be so far removed that no spark passes from pole to pole, the figures appear each separate and complete, but in general branching towards one another. If one pole of the coil be put in connexion with the metal plate beneath the glass and the other be applied centrally to the glass, a figure corresponding to the nature of the pole so applied to the glass is produced. These figures tend to become more symmetrical in development and rounded in outline according as care is taken to centre the pole on the plate and the conductor beneath the plate, and also when the plate used is large. Omitting the underlying conductor diminishes the extent of the figures.

(2.) Of these figures, the + pattern is moss-like and irregular on the edge, the − pattern smooth and cloudy in outline. It is seen that within each of the figures the pattern corresponding to the

opposite sign is located. Thus, near the centre of the positive pattern the cloudy-edged negative pattern is found, and *vice versâ*. More than one internal figure may sometimes, but rarely, be observed. Thus a third figure, corresponding to the external pattern, may occur. Whether these secondary figures are due to a certain amount of oscillation in the current flowing through the circuit or not is not well determined. An experiment in which both the capacity and self induction of this circuit was increased by inserting in it the secondary circuit of a second coil of nearly equal dimensions afforded figures no way differing from those previously produced. It is observable, too, that very large and very small coils give a similar arrangement of the figures. There are other reasons for believing that the major part of the current in a coil discharge is unidirectional, but a small amount of return current might cause the central disturbance on the plate. Again, the central figure may be due to a back discharge from the electrified surface of the plate, as Professor Fitzgerald suggests.

If the inner coats of two Leyden jars be connected with the poles of the coil and so arranged that they can spark to each other across a gap of 4 or 5 cm., the outer coats being separately connected with the wires touching the powdered surface of the plate, the phenomena of discharge differ somewhat. This circuit will afford an oscillatory discharge, and accordingly it is found that triple figures are most frequently formed and that the secondary figure is more conspicuously developed. When the poles on the plate are very near each other, so that a very vigorous spark passes, the secondary figures are seen to encroach on the primary, even branching through them and mixing with them, so that it becomes difficult to distinguish the + from the − pattern. With a wider sparking distance the secondary are indeed contained within the primary, but closely border upon them. And as the sparking distance widens, the secondary figures retreat inwards towards the centres. Finally, when the distance is increased to such an extent that no spark passes, the central figure becomes very inconspicuous; the + becomes very much to resemble the straggling lines of the Lichtenberg figure, and the − becomes an irregular cloud. In these observations it is seen that where the conditions for a vigorous oscillation of the current are favoured the multiple character of the figures grows more marked.

(3.) Various powders were tried in the production of these figures —charcoal, French chalk, very fine emery, mixed sulphur and red lead, the protoxide and peroxide of tin. The last two powders, the oxides of tin, differed in some respects in their behaviour from the others. They exhibited, indeed, faintly, the types of pattern observed with the use of lycopodium, but took in addition a ring-like formation round the poles, the rings being irregular and wavy and sometimes

very numerous. All the other powders behaved like lycopodium, but showed less perfectly developed figures.

(4.) When a plate has been exposed to the action of the spark in producing these dust-figures, the powder which before was loose on the surface of the plate will be found to have become fixed, or to a considerable extent adherent, to the plate where the figure has been formed, and continues to remain so for many weeks; so far as I have observed, indeed, indefinitely.

If a plate bearing a dust-figure be laid by and subsequently (a couple of weeks later) be dusted clean and then breathed upon, breath-figures, differing in the finer detail from those formed on the powder, appear. They more nearly resemble the curious photographic figures obtained recently by Mr. Brown, by passing coil sparks over photographic dry plates ('Philosophical Magazine,' December, 1888). Brisk rubbing, or washing with soap and water, destroys the "magic" qualities of these breath-figure plates. I have not obtained these breath-figures at all so distinctly developed on plates which had very recently been sparked over. It would appear that a certain lapse of time is necessary to confer this quality on the plate. Breath-figures of somewhat similar character have been noticed before.

(5.) Formed in an atmosphere of *coal gas*, the patterns show a marked variation.

In the negative a very regular, halo-like ring surrounds the pole, through which the characteristic cloudy fronds of the negative pattern break out, as it were, in places, extending further on the plate. The + pattern appears in the centre of the halo. The development of spark veins is less conspicuous.

In the positive there is less variation from the normal pattern in air. There are fewer spark veins, and hence less branching. The characters are more those of an irregular outline, with deep mossy edging.

Formed in *hydrogen*, the negative is reduced to a faint, circular halo, with a very faint positive pattern within. No spark veins observable. The positive pattern shows only a few thin, sharp-branching spark veins, fragmentary and radiate to the pole, with an indefinite aggregation of the powder around them.

Formed in *carbon dioxide*, there is no notable change from the figures formed in air. The distinction between the two patterns, the + and —, is perhaps better developed, there being some increased likeness with the Lichtenberg figures.

When the figures are developed under the receiver of an air-pump at *diminished air pressure* it is found that at a pressure of 15″ of mercury the negative form appeared very much as in coal gas, *i.e.*, with a regular halo, having straight radiate marking, few and faint spark veins and a mossy positive pattern near its centre. The positive

form had a mossy outline, with but little branching and few and faint spark veins. Then a negative cloud-edged pattern, with finally a second mossy development of the + form at the centre. At 10″ pressure the — pattern was a very regular halo, with uniform texture. No spark veins, and central mossy + form. The + pattern was a mossy edging, with a second mossy pattern at the centre and an intermediate undisturbed region, in which no definite marking could be detected. At 6″ pressure the halo of the — form is more extended, very faint, and shows the moss pattern at its centre. No spark veins. The + form consists of a few coarse, straggling lines, extending towards the centre, a region within of unmarked powder, and a few more thick straggling lines wandering from the centre. All these thick lines show a central core of unmoved powder, each, in fact, consisting of two parallel lines in which the powder has been removed, leaving an undisturbed central axis. No spark veins.

In air at 3″ pressure both forms have become very indistinct. The suggestion of a halo in the negative: a little pitting here and there, not deep enough to expose the glass, in the positive.

It appears from these experiments that the nature of the gaseous dielectric exerts a considerable influence on the nature of the path taken by the outspreading current. It would seem to be also a question as to the degree of conductivity possessed by the gas. The ring-like symmetry of the negative pattern, as well as the generally more symmetrical outspread of the positive, and the absence of spark veins in both, seem to indicate a uniformity of spread of the current in the better conducting media, as hydrogen, or air, at reduced pressure. This suggests, in fact, that these dust-figures owe their forms chiefly to the manner in which the current spreads in the surrounding gas. It has already been seen that the nature of the powder in general exerts little qualitative influence. The nature of the plate carrying the powder has yet to be dealt with.

(6.) Formed on the surface of a sheet of vulcanite, the figures exhibited no distinguishing feature from those on glass. Thus it appears that a difference of specific inductive capacity (vulcanite is two-thirds that of glass, according to Gordon) does not appear to affect what influence the non-conducting plate may exert on the form of the figure. It will now appear, however, that the isotropic quality or otherwise of the plate is an important factor in determining the path taken by the current.

A plate of selenite cleaved on the clinodiagonal, measuring about 6 × 7 cm., was polished smooth on opposite faces, having been reduced to a thickness of about 4 mm. Owing to perforations, due to loose crystallisation, the crystal had to be laid down on glass with melted paraffin. Dust-figures taken on this crystalline surface showed a very marked variation from those effected on glass. This is espe-

cially observable in the positive figure. The straggling lines of the tufts on this pattern are distorted considerably into the direction of cleavage of the crystal. The outlying "crow's feet," common on these figures, are symmetrically oriented with reference to the cleavage. The entire figure, indeed, evinces unequal development in directions coinciding with the cleavage of the crystal. Wiedemann describes experiments on the conductivity of crystals by discharge from a Leyden jar over dusted surfaces. The dust was thrown into a ring more or less elliptical, according to the degree in which the conductivity of the crystal differed in different directions. ('Poggendorff's Annalen,' vol. 76 (1849), p. 406).

It would be interesting to try the effects on stressed dielectrics. An attempt of my own to deal with stressed glass failed ultimately from want of adequate means of putting a uniform and sufficient stress on the material.

The distortion of the figure produced by an unequally conducting plate shows that the plate, as might be expected, shares in the conduction of the current, which it will do more or less, of course, according to the degree of conductivity it possesses compared with that of the gaseous dielectric. The characters of the figures are, however, probably conferred by the gas, the parts of which being isolated and mobile will tend to favour want of uniformity in the discharge, losing equilibrium under small electrostatic stresses.

(7.) Figures may be formed by inductive action exerted through the dielectric plate.

A glass plate, dusted on *both* sides, is supported about 5 mm. above a smooth metallic surface which is placed in contact with one pole of the coil. The other pole touches the upper surface of the plate centrally. On the current passing a figure is formed on the upper surface corresponding to the pole in contact with it, and on the lower surface a pattern of the opposite kind, but less distinct. Looking through the plate, it is seen that these figures are fairly superimposed, *i.e.*, the outline of the + form corresponds with the outline o the — form. If two glass plates be laid one above another separated by 3 or 4 mm., the lower resting on metal, the figure on the upper surface of the lower plate corresponds in a feeble way to the figure on the upper surface of the upper plate. Similar inductive effects were observed by Mr. Brown in the case of his photographic figures (*loc. cit.*).

(8.) If a plate be dusted on both sides and touched at each side centrally by one of the poles, figures may be formed by direct action on both sides of the plate. The plate may be held in a vertical position. When the dielectric plate is thin the coincidence of the figures so formed is very remarkable. Thus, taken in a plate a couple of millimetres in thickness, every tuft of the pattern on the + side

covers a cloud on the negative. The branching spark veins, too, correspond or overlie closely. Sometimes, however, the coincidence of these latter is not perfect, for, if a plate be dusted and breathed on upon both sides, the spark veins, then showing out more clearly, are seen to diverge a little in some cases.

That this coincidence of development at each side of the plate is inductive and not ascribable to luminous action, possibly initially developed on one side and then determining by its influence discharge along certain paths on the other, is shown by repeating the experiment on red photographic glass, when the coincidence is as striking as before.

It is hard to make out the exact nature of the coincidence. It does not appear to be that of photographic positive and negative throughout. However, the clear margin around the clouds on the − pattern is invariably backed by the marginal frill of the + pattern. It is interesting and curious to observe in the dark this inductive transmission of the current across a sheet of clean glass, arranged as in the above experiment. The close but not perfect coincidence of the spark veins is then easily noticeable.

(9.) If a bundle of plates making up about a centimetre in thickness be arranged as in the last experiment (*i.e.*, the extreme surfaces powdered and touched centrally by the poles), the figures no longer show the coincidence observed in the experiment with the thin dielectric, but are smaller and more of the Lichtenberg type. That is, the + tends to be more straggling and tufted, the − more rounded and lobed. The inductive action is here feebler, and matters are more as in Lichtenberg's experiment.

(10.) On the other hand, diminishing the thickness of the dielectric gives rise to figures of great minuteness and detail. With very thin dielectrics it is difficult any longer to distinguish the + pattern from the −. If such thin dielectrics be laid down on a metallic surface as in the first-described experiment (1), the extreme delicacy of form is still obtained. In this way the figures of Plate 2 were formed upon a sheet of the thin glass used for microscopic cover glasses. Still greater delicacy may be obtained by using thin sheets of mica, but ultimately the piercing of the plate by the spark sets a limit to the experiments. In these experiments the conditions are those for a very strong inductive action. The double nature of each figure is apparent in these as well as in the former figures, and the similarity, or perhaps identity, of the forms of opposite sign is very well seen by comparing the inner with the outer of these patterns.

(11.) Figures taken on opposite sides of a thin insulator at diminished air pressure present some peculiarities. A thin plate of micro-cover glass was arranged to stand vertically beneath the receiver of an air-pump, the poles touching it centrally at each side.

Figures obtained on both sides of this plate at pressures in the receiver of over 15″ of mercury showed much the same minutely divided patterns, in which the + is hardly distinguishable from the —, as occur at ordinary pressures. At about 15″ pressure, however, a sudden transition in the nature of the figures occurs. The negative becomes very indistinct, a hazy ring with little veining. The positive becomes a few long straggling lines, radiate to the centre. So abrupt is the transition that a plate was obtained on which about one-third, or 120°, of the first sort of pattern was definitely replaced by a sector of the second sort, a faint halo on the negative and a ring of straggling radiate lines backing it on the positive side.

As the pressure diminishes the second order of pattern persists and develops. The positive ring spreads outwards, the straggling lines of which it consists becoming shorter. The negative grows so indistinct as to be no longer easily located on the plate; it is found, however, backing the positive ring on close observation. Ultimately both forms disappear from the plate, the positive persisting at pressures at which the negative is quite indiscoverable.

It would appear from all these observations that where the discharge is chiefly in the gaseous dielectric it is to some property of this medium that the peculiar characters of the patterns are to be ascribed. Faraday's view that much of the individualities of positive and negative brushes, sparks, &c., was to be ascribed to the behaviour of the matter conveying the current may possibly be an approximation to the truth in the present case, although affording no insight into the manner in which such a remarkable distinction in the nature of the figures can be brought about by a difference in the behaviour of the gaseous matter towards positive and negative discharges. This view is, however, I think, rather confirmed by the lessening of the distinction between the patterns as the dielectric plate gets thinner, for it is probable that in the case of a thin screen separating the discharges these occur to a greater extent in the matter of the screen, the parts of which, yielding only slightly to stresses, refuse to show any distinguishing behaviour towards positive or negative.

That the distinguishing features of the positive and negative patterns vary with the nature of the matter which shares with the gas in carrying the discharge is apparent, from Mr. Brown's results, in air discharges over photographic plates. The photographic film in this case probably possessed a considerable degree of conductivity, and so modified the air discharge. In the case of the peroxide and protoxide of tin there was also, as already observed, a modifying, or at least a superadded, effect. It is probable that with the use of lycopodium there is with thick plates little modification of the air discharge. The next experiment is a case in point, although much of

the detail observed may result from the fact that the powder was fine and adherent to the glass.

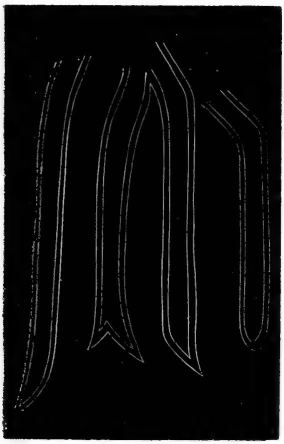
(12.) A glass plate was held over burning magnesium ribbon till a fairly even sublimate of magnesia covered its surface. On this a

FIG. 1.

Discharge over sublimate of magnesia.

discharge was taken, resting the glass on a conductor. It is to be observed, in the first place, that the figures obtained on this surface do not generally penetrate to the glass, but are impressed on the surface of the sublimate. At the same time, the grain is exceedingly fine, bearing examination with a high power under the microscope. Taken on ordinary glass (such as is used in photography), the negative exhibits a centre of fine bent veins and a faint but very beautiful, irregular halo, with wavy border, extending in the direction of the positive pole. The positive most resembles the outspread tentacles of some of the larger sea anemones (fig. 1), but the tentacles, magnified, are seen to have the curious structure sketched in fig. 2. Their extreme

FIG. 2.



Enlargement of tentacles.

points are deeply sunken in the powder, and they sometimes bifurcate at the extremity and curl round, meeting each other in a very peculiar way. Each contains a central core of undisturbed material; they are, in fact, outlined only. On thin glass very delicate figures are obtained. The positive somewhat resembles Mr. Brown's photographs for that pole—veins bordered with innumerable streaming lines. The negative shows a strong resemblance to the positive, being, in fact, a similar pattern with finer streamers. In this last case again we might suppose that the discharge was shared to a greater extent by the sublimate, and differences between the positive and negative gas discharges accordingly reduced.

The undisturbed core in the spark lines has been observed in the case of spark tracks over smoked glass (Töpler). It is developed in breath-figures, or is seen when sparks from a Holtz machine, or from

a jar, are passed over emery dusted on glass. The phenomenon recalls the shallow penetration of transient currents in conductors, and suggests that possibly the inflow of energy from the surrounding medium is not, in these cases of heavy sparks, concentrated to form linear disruption, but disruption on a cylindrical surface.

(13.) To investigate in some measure the effects of the discharge in the substance of the dielectric plate, a plate of ordinary glass was employed, having its upper surface flooded with melted paraffin, kept fluid by resting the glass on a warm sheet of metal. During discharge a ridge or mound of the liquid paraffin was repelled into the region between the poles extending nearly across the plate, and always nearer to the negative pole. Around the poles the paraffin was repelled as if by a strong wind, leaving a circle of glass clean and bare. The circle was largest round the positive pole. Beyond this a radial puckering of the surface of the fluid could be observed. By allowing the glass to cool while continuing the action of the coil, these phenomena could be examined in the solid paraffin. It was curious to observe that as cooling progressed isolated heaps of paraffin gathered from the surrounding fluid, principally near the negative pole, when cold standing 5 or 6 mm. above the general level—possibly a surface tension effect. Short lengths of finely cut up copper wire scattered over the plate while the paraffin was still fluid gathered towards the poles, and, with much gyration and oscillation, set themselves radially to the poles and along lines of flow between the poles. In many cases they struggled into the vacant ring close to the pole, and moved about till they gradually built up lines reaching from the edge of the paraffin nearly to the pole. In some places they set themselves into a kind of network, attaching themselves together, end to end, as if magnetised. These appearances were observed on cooling. Very remarkable are innumerable whirls and eddies to be found on the frozen surface, in some cases so minute as only to be defined with a strong lens.

Similar phenomena were observed using fluid paraffin oil. They reveal the presence of stresses in the dielectric accompanying discharges, which the rigid nature of the glass refuses to show, and suggest the degree to which discharge takes place in the plate.

Some Phenomena Occurring in the Path of Discharge.

The degradation of energy taking place in an electric discharge through a gaseous medium is complex in character. There is development of heat, probably according to the laws of Ohm and Joule; kinetic energy imparted to the molecules by electrostatic repulsion and attraction, and chemical potential energy in the liberated ions. The liberation of ions cannot be supposed to occur in the same manner as in liquid electrolytes. In the latter case the actual expendi-

ture of energy is confined to the immediate surface of the poles. In the spark the expenditure of energy is continuous from pole to pole. The abundance with which ions are liberated precludes the idea that they are liberated only at the poles. This distinction is doubtless ascribed partly to the free motion and isolation of the molecules disintegrated by the spark, partly owing to the explosive disturbance and dispersion of the atoms. For heating, repulsion, and chemical dissociation will unite in setting up a violent rush of atoms and molecules from the axis of the spark outwards, in fact, conferring its explosive character upon it.

The following observations for the most part find their explanation in the mechanical conditions obtaining in the spark path and relate to the behaviour of the spark in passing through confined spaces.

(14.) Two leads of very thin platinum foil are laid down on a slip of glass. The leads are about 30 mm. in length by 1 mm. in width. The glass slip may be an ordinary microscope slip. The leads are laid along the slip in the same straight line, with their ends (preferably cut to points) separated by about 20 mm. at the centre. A piece of thin cover glass is now cut into a rectangular shape, 40×20 mm. *q.p.*, warmed and touched on one side at each corner with shellac. This is laid down centrally on the slip, covering the gap between the leads, and the whole is held over a flame till the shellac between the cover glass and the slip begins to melt. It is then placed on a smooth table and the cover pressed and rubbed down till Newton's rings appear in the space separating the leads. This is easily accomplished if the surfaces have been wiped clean before putting them together. The rubbing is continued till the black spot is produced at the centre of the rings.

If wires from a Ruhmkorff coil are now brought to touch the leads where they extend beyond the cover glass, it will be seen that the spark in its passage across the confined space between the cover glass and the slip refuses to cross the centre of the rings. It makes a detour, curving round at a distance of four or more rings from the black spot, that is, it will sometimes pass in the fourth ring, sometimes in the sixth or eighth. It will often divide into several sparks, some going one side, some going the other. These sparks show no difference in appearance from free sparks. The rings will be seen to be disturbed by the sparks, generally widening as if the glasses came closer. In some cases, however, they soon become destroyed. It is observable, however, that sparks passed through such a narrow space will often produce the rings where none at first existed. Such rings persist for many hours, so that the effect is hardly due to heating, but probably due to an electrostatic straining of the glass.

It appears from this experiment that the spark experiences a higher resistance near the centre of the rings, either because the molecules

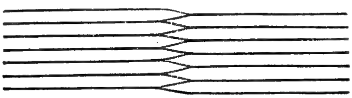
are there constrained in their motion or are fewer in number. If an external by-path be arranged, the spark will often prefer to clear as much as 5 cm. of free air to passing a distance of 1·5 cm. between the glasses. Accurate measurements were not attempted, for with the apparatus used it would evidently have been difficult to have arranged that definite conditions should obtain as to the space between the glasses.

On arranging the slip beneath the receiver of an air pump so that the path of the spark can be observed at low pressures it is sometimes, not always, seen that at low pressures the curvature of the spark increases. It may move outwards by three or more rings, the rings themselves remaining unaltered. When the pressure is much reduced it becomes difficult any longer to keep the spark between the glasses. It chooses then some external path.

(15.) In the last experiments after the first half-dozen sparks were passed through the straitened space between the glasses a faint etched line becomes apparent at the place where the constraint was greatest, *i.e.*, close to the rings. The formation of this line may be observed. It is accompanied by a local brightening of the spark and an emission of sodium tinted rays as if the glass was being volatilized. The line will preserve the curve described by the spark. It may be as long as 4 or 5 mm. or merely a speck.

On placing this under the microscope the line is resolved into a very beautiful and regular structure, diatom-like in its delicacy of marking. Fig. 1, Pl. 3 is a photograph of such a spark track magnified 64 diameters. The width of the track is about one-tenth of a millimetre, but this is slightly variable. Measurements of the side rays show that they are from 330 to 420 to the millimetre. Such a track may be deepened and lengthened by repeatedly passing the spark, the side rays remaining apparently undisturbed in position, but widening a little and lengthening a little, so that a track which is a very faint impression when first examined may be submitted again to the current and gradually brought up. It will be found too in this process that the pitted central core of the track widens and sinks deeper into the glass. The contrast between a fresh and a worn track is interesting. In the former none of the central pitting is developed, but the side rays meet at the centre somewhat as shown in the cut (fig. 3). If the glasses be taken asunder it will be

FIG. 3.



Formation of spark tracks.

found that both glasses have shared in the marking, seemingly to an equal extent. The marks, too, are apparently *vis à vis.*

What has caused this structure? The answer is, I think, not hard to find. In the first place these marks are melted in the glass. Their rounded contours and ridges leave no doubt of this. They are quite distinct in appearance from the hair-like cracks to be observed extending from the track at more or less regular intervals. If the spark be regarded as a line of explosion from which atoms raised to a high temperature are being repelled, the effects observed are quite explicable. These atoms, in the free spark escaping all round, are here compelled to make their escape by the narrow side ways only. The side rays depict the lanes of escape cut by the heated, outrushing molecules. Shift the slide under the microscope till the extremity of track is approached. Here the "head-room" is increasing. Fig. 2, Pl. 3, is a photograph of such a part of the track. Here there is a relief of pressure in the longitudinal direction. The outrush becomes more axial along the spark. Not quite axial, because there is also a possible sideway escape and the lines brush out in the intermediate direction of easiest escape. Hence the tracks generally end up in these beautiful feathery forms.

Occasionally the spark reaches a point in its path where side escape is difficult or impossible. It then assumes the appearance of fig. 1, Pl. 4. The central part is much pitted and lines of escape appear diverging at each end. This is perhaps in some respects remarkable. It might be thought that the unidirectional current would polarize the motion of the atoms, but it would appear as if the atoms, having received their energy from the ether, were left uninfluenced sensibly by any stress or motion in the medium to polarize their movements, other than their tendency to move from the spark axis in a radial direction. In fact the directions of longitudinal flow are apparently obedient to conditions of pressure only, or seem at least sufficiently explained by such.

The lines on the concave side of a sharply curved track are shorter by a little than those on the convex side. The reason of this is that escape is not so free on the inner side. Passing round sharp obstacles accidentally occurring in the path, the axis of the spark track is close in near the obstacles, the lines chiefly radiating outwards.

I made up a slide in which a couple of very fine lines had been engraved with an etching diamond on the surface of the slip, extending across the field at right angles to the path of the spark. On passing sparks it was found that for a little distance at either side of the point where the track crossed these lines the side rays were discontinued, evincing the relief of pressure afforded by the channels.

When a spark bifurcates the course of the outrushing atoms is

shown in (fig. 2, Pl. 4). It is probable that in this figure we are looking at a point on the spark track where a considerable outflow of atoms initiated a divergence of the spark or a bifurcation of it.

(16.) To investigate the nature of these tracks a slide was experimented with when exposed to the reduced air pressure of 10″ of mercury. The spark tracks produced at this pressure were found devoid of the side rays. Instead, beads of melted glass had arranged themselves along the centre of the track, some drawn out into long streaks, others globular. Another slide at 12″ pressure showed very rudimentary development of the rays. At 20″ some rays were obtained, but all these slides treated at ordinary pressures rapidly developed side rays, sometimes bordering the old tracks. The low-pressure tracks, it is to be observed, often develop a budded appearance at irregular intervals, as if the accumulated pressure was content with an occasional relief. The buds open outwards from the sides and ends of the track, fanwise. These are often quite smooth, but develop grooved lines or rays when treated at ordinary pressures. They, in fact, experience a more intense outrush of matter at high pressures than at low pressures.

Why a track can be intensified by repetition of the spark is easily understood. The channels of escape once indicated by the faintest grooving will naturally continue their functions at the passage of each spark, and in this way become deepened.

(17.) It has frequently been shown that on glass there is a surface condensation of moisture. To find if this had a part in the production of the rays, slides were made up of glasses which had just cooled after being heated to redness over a Bunsen burner. The results of several experiments showed that the rays were obtained without difficulty, but perhaps not quite so readily as on slides which had only been rubbed clean in the usual way. The effect of the surface layer of moisture is slight, almost inappreciable.

(18.) The regular spacing of the rays might suggest that the free path of the atoms or molecules might determine in some degree their spacing. To test this question the slides were arranged for experiment in an atmosphere of hydrogen. Great care was taken by a preliminary heating of the glasses composing the slides and subsequent frequent exhaustions, after each exhaustion admitting hydrogen around the slide, to withdraw all air from the space between the glasses. The tracks obtained in this atmosphere of hydrogen turned out to be very similar, generally, to those obtained in air; in places quite indistinguishable from them. In some places, however, a striking distancing or widening of the side lines is observed. The lines here are thicker, shorter, and gapped, *i.e.*, one ray is not continued unbroken from the axis, but is divided by minute gaps. Measurements showed such lines to be spaced about 250 to the milli-

metre. But again measurements in other places, where the spacing was finer, gave similar numbers to those obtained in the case of air, rising to 420 to the millimetre.

It seems probable, in the first place, that some air will linger in these narrow spaces. This might explain the occurrence of the finer spacing. However, granting this, it is not certain that a wider spacing of the lines in the case of hydrogen may not be explained on its superior conductivity or inferior density. These qualities might very conceivably influence the intensity and effects of the outrush from the spark axis. The spacing of the lines might, in fact, be considered as dependent on the accumulation of pressure at points along the axis of the spark. When this accumulation at any point becomes sufficiently intense it breaks out, cutting a channel. It might easily be supposed that such points, where the conditions were uniform along the path of the spark, would be very evenly spaced. The distance separating such points might depend on many properties of the gas, as its conductivity, specific heat, density, and pressure. There is not then, I think, sufficient reason to suppose that there is anything in common between these marks and the striæ observed in vacuum-tubes. The explanation just given seems quite adequate to explain the formation of the marks, but no mere modification of such an explanation will fit in with many of the observations on striæ. The conditions obtaining are really quite different in the two cases.

(19.) Whether the period of electric oscillation of the spark path and leads had an influence in determining the spacing of these lines, was investigated by an experiment on a slide in which the dimensions had been altered till nearly double the ordinary size. The tracks obtained in this, however, presented no peculiarities.

(20.) Observations on the appearance of the spark, confined between the glasses, in the field of the microscope, using a magnification of about 60 diameters, revealed no visible peculiarity. Nor did the use of a wedge of neutral tinted glass, introduced to lessen the dazzling light, enable any detail to be grasped. When, however, passing in a narrow space, the sparks could be seen rapidly melting the glass. Photography of the sparks thus magnified was also tried. The photographs show a bright central part, hazy border, and—very faintly—flame-like streamers extending rectangularly from the border and to some two or three diameters of the sparks at either side.

(21.) The following experiment is explained in an outrush of matter *uniformly* from all points along the spark. Into a piece of thermometer tubing about 0·2 mm. in bore and about 4 cm. in length, two leads of very fine, straight platinum wire are laid loosely. They are separated in the tube by about 1 cm. Let this gap be situated at a distance of 1 cm from one end, and therefore 2 cm. from the other,

and connect with a coil so that sparks pass. If now a stream of coal-gas be passed across the end of the tube nearest the spark, two-thirds of the length of the spark near this end will become coloured with the blue tint assumed by a spark in coal-gas at atmospheric pressures, the remainder of the spark length remaining as before. Altering the direction of the current does not influence the result. If the coal-gas be passed across the other extremity of the tube, the one-third of the spark length near that end becomes blue, the other two-thirds continuing an air spark.

Move the spark gap into the middle of the tube and repeat the experiment. Now it is seen that *half* the spark turns blue when the gas is approached to one end; always that half nearest the end opening into the current of gas.

If finally, a flame of any kind be approached one end of the tube, at each spark the flame is seen to be blown outwards as if acted on by a blow-pipe. The explanation of the coloration of the coal-gas is to be found in a back suction of gas occurring after each explosion, the succeeding spark becoming coloured according to the distribution of gas in the tube. This is a question of facility of egress and ingress. When the spark is centrally placed in the tube these are equal at each end, and half the spark will be coloured. In the unsymmetrical position of the spark, outflow and inflow are facilitated at one end and obstructed at the other according as the spark is located nearer the one end and further from the other.

When a spark is caused to pass in the centre of a tube so long as 10 or 12 cm., the puffing out of a flame applied at the end may still be observed, but there is no coloration of the spark when coal-gas is passed across the end. It fails to penetrate so far along the tube as to reach the spark.

The partial coloration of the spark, taking place in this very symmetrical way, must evidently depend on a uniform repulsion of matter along the length of the spark. Were the molecules, for example, only repelled close to the positive pole the symmetry of effects would hardly obtain.

(22.) In addition to the fine rays bordering the spark tracks in more worn tracks, the arrangement of the melted glass is noteworthy. Very often three strings of glass beads are discernible, a central one of large beads or globules, spherical or elongated, and a string of very small spherical globules extending along either side. All are contained in a smooth trough-shaped track. Occasionally the globules are, however, differently arranged. Thus the tiny somewhat V-shaped marks, all pointing one way, shown in (fig. 1, Pl. 5), consist of very minute beads; the largest at the apex, the smaller streaming backwards and outwards like the wash from the bow of an advancing boat. Observations show that this arrangement

may occur reversed in order in the same slide, *i.e.*, the pattern may point towards either pole.*

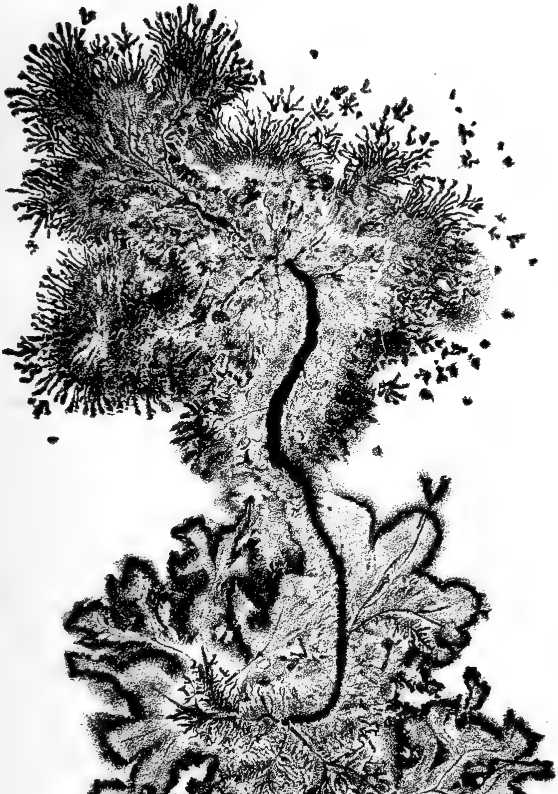
(23.) A few of the cracks which cross the spark tracks are seen on the last figure and on the other figures. When the spark traverses wider places cracks alone appear, and these are sometimes very regular in appearance. It would appear that these are the result of electrostatic stresses in the glass exerted orthogonally to the lines of flow. Thus, near the poles these cracks curve round the pole with a small radius of curvature, the curves flatten as they are further removed from the pole and in the centre of the field cross the line joining the poles perpendicularly. Intersecting these orthogonally are the spark tracks, which, in cases where the glasses are at a uniform but short distance from one another throughout, or parallel, appear to pass in elliptical lines from pole to pole. Fig. 2, Pl. 5, is a photo magnified fifteen diameters of the field near the positive pole. It will be seen that it recalls the usual figures of the distribution of equipotential lines and lines of flow for such a disposition of the poles. It is remarkable that near the negative pole the flow lines often curve outwards at some little distance from the pole. If free sparks, taken from carbon points or dusty terminals, be observed, the luminous particles of burning carbon or dust will be seen to have the same outward curvature at the − pole. Those at the + pole curve elliptically towards the negative (fig. 4).

FIG. 4.

Path of incandescent particles at the poles.

(24.) The homogeneousness of the glass is essential to all the foregoing effects. A plate of Iceland spar was polished parallel to a rhombohedral cleavage face. On this a thin cover glass was laid down over platinum leads as before and sparks passed. Search for tracks of the usual appearance on this was in vain; different phenomena presented themselves. The track was marked by innumerable little pits with raised, rounded edges, running on the whole in cleavage directions, *i.e.*, dividing up the surface into rhombohedral pits. These pits look as if they had been melted into

* The cloudy marking at either side of the track seen in the figure is due to moisture which had penetrated between the glasses before the photograph was taken. In the case of the other photographs the glasses were separated before photographing, the figures being obtained from one of the glasses only.



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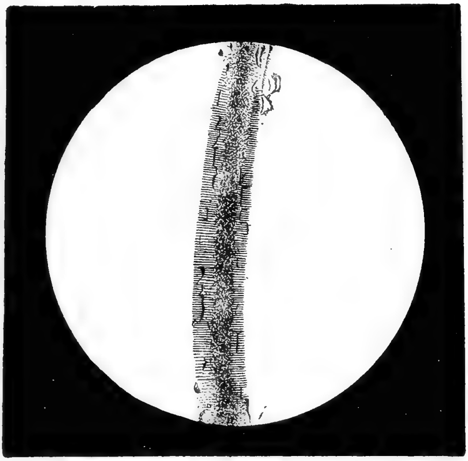

Fig. 1.

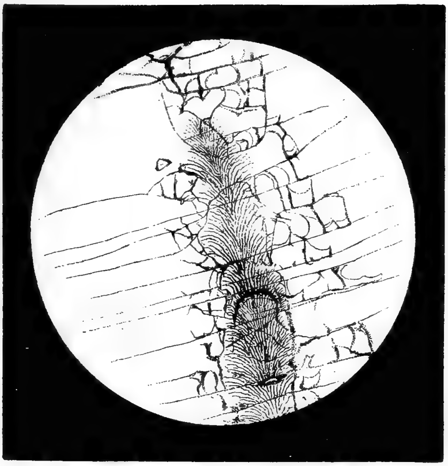

Fig. 2.

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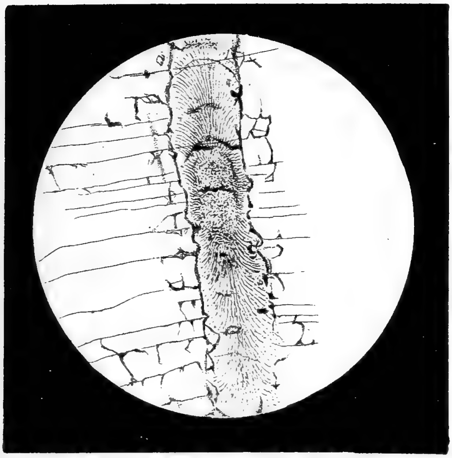

Fig. 1.

Fig. 2.

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Fig. 1.

Fig. 2.

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the surface of the crystal. Fine cracks, set along the spark tracks, quite distinct from the pitting, and bent in cleavage directions, add to the curious appearance in the field of the microscope. Sometimes a track approximating to those obtained on glass is met with, but the side rays are bent and distorted. I found, also, that the appearance of the tracks varied with the direction which the spark took in reference to the cleavage lines.

A similar experiment on a plate of selenite gave like results, the pits being more elongated in accordance with the more acute cleavage intersection. Mica plates showed a fine net of hexagonally arranged lines covering the surface where the spark had passed. Further experiments in this direction might be of interest, but other work has hindered me from pursuing the subject.

(25.) Sparks from the Leyden jar will produce tracks on glass similar to those described in the foregoing; but experimenting with such sparks is difficult, as their very explosive nature leads to a rapid break up of the cover glass.

January 9, 1890.

Sir G. GABRIEL STOKES, Bart., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "New Experiments on the Question of the Fixation of Free Nitrogen. (Preliminary Notice.)" By Sir J. B. LAWES, Bart., LL.D., F.R.S., and Professor J. H. GILBERT, LL.D., F.R.S. Received (in part) and read January 9, 1890.

Received January 9.

In a paper presented to the Royal Society in 1887—1888, and printed in vol. 180 of the 'Philosophical Transactions,' we discussed the history and the present position of the question of the sources of the nitrogen of vegetation. We referred to the conclusions arrived at about thirty years ago from the results of Boussingault and from those obtained at Rothamsted, up to that time. We gave the results of some experiments which had been recently made at Rothamsted in connexion with the subject, and reviewed the evidence and conclusions of others published within the last few years.

It was considered that the earlier results obtained by Boussingault,

and at Rothamsted, under conditions in which the action both of electricity and of microbes was excluded, were conclusive against the supposition that, under such conditions, the higher chlorophyllous plants can directly fix the free nitrogen of the atmosphere, either by their leaves or otherwise. Others have, indeed, come to the conclusion that at any rate some plants do directly fix the free nitrogen of the air by their leaves. We believe, however, that there is, up to the present time, no evidence which can be held to be conclusive in favour of such a view.

It was pointed out how large was the store of already existing combined nitrogen in many soils and subsoils, and evidence was adduced to show that even *Leguminosæ* derive, at any rate a considerable amount of nitrogen from nitric acid within the soil and subsoil; and, further, that it was, as a rule, those having the most powerful root-development that took up the most nitrogen from somewhere; and it was considered that this fact pointed to a subsoil source.

Upon the whole it was concluded that, at any rate in the case of our gramineous, our cruciferous, our chenopodiaceous, and our solaneous crops, atmospheric nitrogen was not the source. It was admitted, however, that existing evidence was insufficient to explain the source of the whole of the nitrogen of the *Leguminosæ*.

According to some of the more recent experimenters, the fixation of free nitrogen is not limited to our leguminous crops; and the modes of explanation of the gains of nitrogen observed are extremely various. Thus, it is assumed—that combined nitrogen has been absorbed from the air, either by the soil or by the plant; that there is fixation of free nitrogen within the soil by the agency of porous and alkaline bodies; that there is fixation by the plant itself; that there is fixation within the soil by the agency of electricity; and, finally, that there is fixation under the influence of micro-organisms within the soil, with, or even without, the accompanying growth of higher plants.

The balance of the evidence recorded seemed to be undoubtedly in favour of the supposition that there is fixation under the influence of micro-organisms or of other low forms within the soil, and of all the various results which were discussed, those of Hellriegel and Wilfarth were considered to be by far the most definite and significant; pointing to the conclusion that, although the higher chlorophyllous plants may not directly utilise the free nitrogen of the air, some of them, at any rate, may acquire nitrogen brought into combination under the influence of lower organisms, the development of which is, apparently, in some cases, a coincident of the growth of the higher plant whose nutrition they are to serve. Such a conclusion is, however, of such fundamental importance that it seemed very desirable that it should be confirmed by independent investigation.

Accordingly, as stated in a postscript to our paper, dated October, 1888, it had been decided to institute experiments at Rothamsted on similar lines, and a preliminary series was then in progress. A second and more extended series has been conducted in the past season, 1889. It is proposed to give, on the present occasion, a description, and some of the numerical results, of the experiments made in 1888, and a description, and some illustrations, of the growth in those of 1889.

It was in 1883 that Hellriegel commenced a comprehensive series of vegetation experiments in pots, in which he grew agricultural plants of various families in washed quartz sand. To all of the pots nutritive solutions containing no nitrogen were added; to one series nothing else was supplied; to a second a fixed quantity of nitrogen as sodium nitrate; to a third twice as much; and to a fourth four times as much was added. The result was that, in the case of the Gramineæ, and some other plants, the growth was largely proportional to the nitrogen supplied, whilst in that of the Papilionaceæ it was not so. In the case of these plants, that of peas for example, it was observed, however, that in a series of pots to which no nitrogen was added, most of the plants were apparently limited in their growth by the amount of nitrogen which the seed supplied; whilst here and there a plant growing ostensibly under the same conditions would develop very luxuriantly; and, on examination, it was found that whilst no nodules were developed on the roots of the plants of limited growth, they were abundant on those of the plants that grew luxuriantly.

In view of this result, Hellriegel instituted experiments to determine whether, by the supply of the organisms, the formation of the root-nodules, and luxuriant growth, could be induced, and whether by their exclusion the result could be prevented. To this end, he added to some of a series of experimental pots 25 c.c., or sometimes 50 c.c., of the turbid extract of a fertile soil, made by shaking a given quantity of it with five times its weight of distilled water. In some cases, however, the extract was sterilised. In those in which it was not sterilised there was almost uniformly luxuriant growth, and abundant formation of root-nodules; but with sterilisation there was no such result. Consistent results were obtained with peas, vetches, and some other Papilionaceæ; but the application of the same soil-extract had no effect in the case of lupins, serradella, and some other plants of the family which are known to grow more favourably on sandy than on loamy or rich humus soils. Accordingly, he made a similar extract from a diluvial sandy soil where lupins were growing well, in which it might be supposed that the organisms peculiar to such a soil would be present; and, on the application of this to nitrogen-free soil, lupins grew in it luxuriantly, and nodules were abundantly developed on their roots.

The Experiments at Rothamsted in 1888.

This preliminary series comprised experiments with peas, blue lupins, and yellow lupins. The peas were grown—

1. In washed sand, with the ashes of the plant added; but no supply of combined nitrogen beyond a small determined amount in the washed sand and that in the seed sown.

2. In similarly prepared sand, but seeded with 25 c.c. of the turbid watery extract from a rich garden soil.

3. Duplicate of No. 2.

4. In the rich garden soil itself.

Each of the two descriptions of lupin was grown—

1. In sand prepared as for the peas, but with lupin-plant-ash instead of pea-plant-ash added.

2. In the same washed sand, &c., but seeded with 25 c.c. of the turbid watery extract from a sandy soil where lupins had grown luxuriantly.

3. In the lupin sandy soil itself.

4. In rich garden soil.

The twelve pots were arranged in a small greenhouse; and distilled water, free from ammonia, was used for watering.

The sand employed was a yellow sand from Flitwick, in Bedfordshire, and was of the same description as is used by gardeners in the neighbourhood for potting. It proved, however, not to be a very pure sand. Thus, after the stones and coarser portions had been removed by sifting, the remainder was several times washed, first with well-water and afterwards with distilled, the turbid washings being poured off; yet it was found to contain after being dried finally for a short time in a water-bath, and mixed with the plant-ash as mineral food, nitrogen as under:—

	Per cent. nitrogen.
Determined by soda lime	0·00287
Determined by copper oxide........	0·00245
Mean..............	0·00266

The sandy soil in which lupins had grown, and from which the watery extract was made for seeding the pots where the lupins were to grow, was still less pure; and it, of course, was not washed, and was only dried at about 24° C.; and, excepting that visible organic matter was removed by sifting and picking, it was used in its natural state as received. Duplicate determinations of nitrogen were made by soda-lime, in the lupin sand alone, and as used after mixture with the lupin ash. The following are the mean results, in each case, calculated on the dry sand:—

	Per cent. nitrogen.
In lupin sand, alone	0·0863
In lupin sand, with blue lupin ash.......	0·0826
In lupin sand, with yellow lupin ash	0·0888
Mean................	0·0859

It may be stated that, in this country, lupins are only grown as an agricultural crop, as food for sheep, on poor, sandy soils, on which little or nothing else will grow. The sand obtained for the purposes of the experiments was from land which had been reclaimed from a common in Suffolk, and on which no corn crop would grow; but on which, when subsequently sown with blue lupins, they had grown as high as the hurdles. It is stated, however, that lupins grow better on good land, but that they are grown on sandy wastes because they will thrive on them when no other crop will.

The garden soil, in the condition as analysed, contained 10·12 per cent. of moisture, and two determinations of nitrogen by soda-lime gave 0·3902 and 0·3936, mean 0·3919, corresponding to 0·4360 per cent. on the soil dried at 100° C.

The pots used were made of glazed earthenware; and were about 7 inches high, 6 inches in diameter at the top, and $5\frac{1}{2}$ inches at the bottom, inside. They had a hole half an inch in diameter at the bottom for drainage, and another at the side near the bottom, into which, outside, a glass tube bent upwards was fixed for aëration; the tube being lightly closed with cotton-wool to prevent insects getting in. The pots rested on slips of thick sheet glass, placed in basins of the same glazed earthenware as the pots.

The mineral nutriment used was as follows:—For the peas. a mixture of 6 parts of pea-straw-ash and 1 part of pea-corn-ash; for the blue lupins, a mixture of 3 parts of blue lupin-straw-ash and 1 part of blue lupin-corn-ash; for the yellow lupins, a mixture of 4 parts of yellow lupin-straw-ash and 1 part of yellow lupin-corn-ash. In each case the greater part of the mixed ash was suspended in distilled water, and sulphuric acid added until there was a slight acid reaction; the rest of the ash was then added, and the whole evaporated to dryness and re-ignited. The ash was then very slightly alkaline to litmus. The so-prepared ashes were mixed at the rate of 0·5 per cent. with the greater part of the sand put into the pots; the remainder of the sand at the top of the pot being without ash.

The drain hole at the bottom of each pot was loosely covered with a piece of thick glass, 1 lb. of broken, washed, and dried flint was then put in, next the sand with ash, and lastly the sand without ash. The pots held from 7 to 9 lbs. of the yellow Flitwick sand, from 6 to 7 lbs. of the lupin sand, and about $4\frac{1}{2}$ lbs. of the garden soil.

The soil extracts, supposed to supply the organisms, were made by shaking, in a large stoppered bottle, 1 part of the garden soil or lupin sand with 5 parts of distilled water; and after the heavier portions had settled down, syphoning off the turbid liquid, which was then passed through platinum gauze to separate any floating matter. The liquid was again shaken before taking the quantity required for seeding the soils, or for analysis. Determinations of nitric nitrogen by Schlœsing's method, and of total nitrogen by copper oxide, gave the following results:—

	Per cent.		In 25 c.c. extract.	
	Nitric nitrogen.	Total nitrogen.	Nitric nitrogen.	Total nitrogen.
	Per cent.	Per cent.	Milligram.	Milligram.
Garden soil extract....	0·000371	0·003159	0·093	0·790
Lupin sand extract....	0·000110	0·001184	0·028	0·296

It is thus seen that the 25 c.c. of the garden soil extract used for seeding contained little more than $\frac{3}{4}$ of a milligram, and the 25 c.c. of the lupin sand extract little more than $\frac{1}{4}$ of a milligram of nitrogen; quantities which are quite immaterial considered as a supply of combined nitrogen.

The Seeds.

The peas were of the description known as Maple field-peas. Four lots, each of 100 seeds, weighed 27·554, 27·460, 27·218, and 27·506 grams; giving an average weight per seed of 0·2743 gram. A large number of single seeds was then weighed, and those only retained for sowing or analysis which gave within 5 milligrams of the mean weight.

In the case of the blue lupins the largest and smallest seeds were picked out and rejected. Of the remainder, four separate lots of 100 each weighed 19·2290, 19·9215, 18·7960, and 19·4580 grams, giving an average weight per seed of 0·1935 gram. A large number of single seeds was then weighed, and those only kept for use the weight of which was within 5 milligrams of the average weight.

From the yellow lupin seed the largest were removed by sifting, and the smallest and those of a dark colour were picked out. Of the remainder, three separate lots of 100 each weighed 12·1060, 11·9640, and 11·6180 grams, giving an average weight per seed of 0·1190 gram. From these, seeds were selected for use which weighed within 5 milligrams of the average weight.

Determinations of dry matter, and of nitrogen, in the seeds, gave the following results:—

	Dry matter at 100° C.	Nitrogen.					
		In fresh.				In dry matter.	
		By soda-lime.			By copper oxide.	By soda-lime. Mean.	By copper oxide.
		Expt. 1.	Expt. 2.	Mean.			
	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.
Maple peas..	93·26	3·537	3·621	3·579	3·531	3·837	3·787
Blue lupins..	94·03	5·105	5·098	5·101	5·364	5·425	5·705
Yellow lupins	94·63	6·649	6·569	6·609	6·404	6·984	6·767

It should be stated, as applicable to the whole of the results as well as to those recorded in the foregoing table, that nitrogen was determined by burning in a vacuum with copper oxide, and collecting and measuring the nitrogen and nitric oxide. In all cases, however, where there was sufficient material, determinations were also made by the soda-lime method, as a check. Nitrogen as nitrates was determined by Schlœsing's method. The copper oxide determinations given in the table, which are those used in the subsequent calculations, were made upon three or four of the average selected seeds, ground up with the copper oxide; whilst the check soda-lime determinations were made on quantities taken from a bulk of ground seeds.

The Vegetation Experiments in 1888.

It was intended to commence the experiments early in the summer, but the pressure of other work and the preparations necessary for the experiments themselves, prevented the sowing of the seed until early in August. Nevertheless, the results obtained in this initiative series were not only of value as affording experience on various points, of which advantage has been taken in the conduct of the more extended series made in 1889, but, as will be seen, they have afforded important evidence on the main point of enquiry itself.

The broken flints, the sand with ash, and the sand without ash, or the garden soil, as the case may be, were weighed and put into the respective pots at the laboratory, taken to the glass-house on August 4, and watered with ammonia-free distilled water. All the seeds were sown on August 6. Three accurately weighed seeds were put into each pot.

From the first, the peas germinated and grew well in each of the

four pots; but in each of the four pots of blue lupins, and in each of the four of yellow lupins, one or more seeds failed, and had to be replaced; and in some cases these also failed. There was in fact failure, not only in the poor Flitwick sand, but in the less poor lupin sand, and also in the rich garden soil. It is admittedly very difficult to secure healthy growth with lupins in pots. On discussing the matter with Hellriegel, at the meeting of the *Naturforscher Versammlung*, at Cologne, in September, 1888, he stated that it had required the experience of several years to insure favourable growth of lupins under such circumstances; and that one essential condition seemed to be that the soil must be kept open and porous; a result which, even with sand, was seldom attained if the dry materials were put into the pot, and then water poured on; the better plan being to bring the sand to a proper condition of moisture by well mixing water with it by degrees out of the pot, and then putting it lightly into the pot. It is also important that the mineral matter added to the soil should be quite neutral.

The failures are well illustrated by the photographs exhibited. Thus, in spite of the re-sowings, there were, on November 3, that is after three months since the first sowing of blue lupins, three plants in pot 1, with the yellow Flitwick sand without soil-extract; only two in pot 2, with the same sand and soil-extract seeding; none in pot 3 with the lupin sand itself, from which the soil-extract was prepared; and three, but of very varying size, in pot 4, with garden soil.

Then the photographs of the yellow lupins show that, in pot 1, with the yellow Flitwick sand, there remained only two plants; in pot 2, with the same sand and lupin soil-extract seeding, only two; in pot 3, with the lupin sand itself only two; and in pot 4, with the garden soil, only two plants.

We shall call attention to the development of the roots, and of nodules on them, in the case of the blue and yellow lupins, further on. Nitrogen determinations have also been made in most of the products; but, as with both blue and yellow lupins, there was actually less growth with than without the lupin sand extract, assumed to supply the organisms, we do not propose to discuss the analytical results on the present occasion; but, so far as that part of the subject is concerned, we shall confine attention to results relating to the peas, of which the growth was much more satisfactory, and the analytical results afford very important indications.

As already said, the peas in each of the four pots germinated and grew well. Throughout, those in the garden soil were more luxuriant than those in either of the other pots. Pots 2 and 3 were each seeded with 25 c.c. of the garden-soil-extract on August 13, that is just a week after the sowing of the seed. For some time, however, the plants in pot 1, with the sand without soil-extract, showed more

growth, and better colour, than those in either pot 2 or pot 3 with the soil-extract seeding. Indeed, it was not until about the middle of September, that is four or five weeks after the seeding with soil-extract, that the plants in pots 2 and 3 began to show a darker green colour than those in pot 1 without the soil-extract. The indication was, however, so striking, that on September 25 it was decided to count the leaves, and to estimate the relative area of leaf-surface, on the plants in the different pots. For this purpose, the leaves were classified into those which were dead, those that were dying, those which were changing colour, and those which were still bright green. It must suffice here to show the number, and the estimated relative area, of the total leaves in each case, on September 25, when the first counting and estimates were made, on October 17, on November 14, and on December 14, when the plants were cut. The following table summarises these results. The first four columns show the total number of leaves, and the second four the estimated relative leaf-surface, that of the plants of pot 1 (without soil seeding) on September 25, being taken as 100.

Peas, 1888.

	Number of leaves.				Estimated relative leaf-surface.			
	Pot 1.	Pot 2.	Pot 3.	Pot 4.	Pot 1.	Pot 2.	Pot 3	Pot 4.
September 25	144	140	120	164	100	67	58	128
October 17	188	200	184	216	143	172	158	242
November 14	244	300	244	280	170	249	245	328
December 14	382	540	390	434	267	481	434	463

It is thus seen that, on September 25, after it had been observed that the plants in pots 2 and 3, with the soil-extract seeding, had begun to show a darker green colour than those in pot 1 without the soil-extract, they nevertheless, up to that date, showed both a less number of leaves, and considerably less leaf-surface, than the plants in pot 1. It is not very clear why the plants with the soil-extract seeding should have remained so long in a comparatively backward condition. It may be that the result was only accidental, depending on the character of the seeds, or on the fact that pot 1 stood at the southern end of the row, and nearest the glass. The alternative is that, in the early stages of development of the organisms supplied in the soil-extract, and of the resulting nodules, the growth of the main plant was, in some way, retarded. The figures show, however, that, from

this date, the plants in pots 2 and 3 with the soil-extract, gradually gained upon those in pot 1 without it, both in number of leaves, and in leaf-surface; until, when the plants were taken up on December 14, those in pots 2 and 3 showed 540 and 390 leaves, against only 382 on those in pot 1; and the plants of pots 2 and 3 showed 481 and 434 of leaf-surface, against only 267 in pot 1. It is seen that there is here clear evidence of increased growth under the influence of the soil-extract seeding.

Photographs of the 4 pots of plants were taken on September 1, on September 22, on October 6, and lastly on November 3, about five weeks before the taking up of the plants, and they indicate relative progress consistently with the estimates given in the foregoing table.

In regard to the general character of growth it should be stated that, in all the pots, the upper portions of the plants obviously developed at the expense of the lower; the leaves of which gradually lost colour, and died off, whilst the stems and the leaves of the upper portion increased in growth; those in pots 2, 3, and 4, continuing to vegetate, and to maintain their bright green colour, up to the end; whilst those in pot 1 had shown more exhaustion, and maintained much less colour. There was, however, as was to be expected so late in the season, no indication of flowering in any of the pots.

It should be further stated, that the plants in all the pots commenced rather early to show signs of mildew, which increased very considerably, especially on the lower portions of the plants, in the later stages of growth. This was, perhaps, not to be wondered at, considering that the greenhouse was in the midst of allotment gardens, and that the plants were unavoidably subjected to considerable changes as to temperature and moisture of the atmosphere. Ventilation was, however, secured as far as practicable.

The next point to consider is, the actual and comparative development of the roots, and of nodules on them, in the different pots, with their different soil conditions. As the roots had to be preserved without any loss, for analysis, the mode of dealing with them for the purposes of examination had to be very carefully considered, and was necessarily more restricted than if examination had been the only object. After the above-ground growth had been cut off and removed, the pots, with their moist soil and roots, were kept in a warm dry place until the examination commenced. The block of soil was carefully turned out on to glazed cartridge paper, with as little shaking or disturbance as possible, and notes were at once taken as to the distribution of the roots, so far as it was then apparent. The sand or soil was then removed little by little, until the roots were left nearly bare. Further notes being then taken, the remaining sand or soil was removed as far as possible by washing in a beaker

with a little distilled water. The roots were then spread out upon paper, and so photographed, and finally noted upon.

Enlarged photographs of the roots of the plants grown in pot 1, with the yellow sand without soil-extract seeding, in pot 2, with the same sand and soil-extract seeding, and in pot 4, in the garden soil, were exhibited.

The roots in pot 1, with the yellow sand without soil-extract seeding, showed a densely matted mass of fibre, those of the different plants being considerably interwoven; and although a few fibres reached the bottom of the pot, and distributed through the flints, by far the greater portion was accumulated within the top 4 inches of the sand; and, notwithstanding there was here no soil-extract seeding, there were many nodules on the roots, but they were fewer, and generally much smaller, than on the roots grown with soil-extract seeding; they were also less characteristically accumulated near the surface, and more distributed along the root-fibres. There were, however, some agglomerations of nodules. Comparing this result with that obtained in 1889, with a purer and sterilised sand, there can be little doubt that the development of nodules, and the comparatively luxuriant growth, in this pot without soil-extract seeding, are to be attributed to the impurity, and non-sterilisation, of the sand.

The roots in pot 2, with soil-extract seeding, also showed a dense mass of fibre, which, however, extended from the top to the bottom of the soil, penetrated the layer of flints, and distributed over the bottom of the pot. In fact, the roots were much more generally distributed throughout the soil, and less accumulated within the surface layers, than in pot 1. The most developed root of the three, had three large agglomerated nodules, each with some scores of protuberances, somewhat as on a raspberry or mulberry. The other plants also showed similar nodules, but of a smaller size. There were also a number of small clusters distributed over the rootlets, but very few single nodules, differing in this respect from the character of development observed in pot 1.

In pot 3, also with soil-extract seeding, each of the three plants had developed a mass of root-fibre extending throughout the soil from the top to the bottom; though the greatest quantity was within the first 6 of the $7\frac{1}{2}$ inches of depth. There were large agglomerations of nodules on the roots of each plant. There were, besides, many small clusters, and here and there single nodules. By far the most of the nodules were within the top 3 inches of the sand; but one considerable bunch was found as low as 4 inches from the surface. As in the other cases, the nodules were grey, and much lighter in colour than the roots on which they grew.

Each of the three plants in pot 4, with the garden soil, had a

stouter main root than any of those in the other pots. From the side branches there proceeded a large amount of fine root-fibres, which extended throughout the whole soil, those from the different plants being much interwoven. The roots extended round the sides and along the bottom of the pot, much more than in either of the other pots. A photograph was, therefore, taken of the block of soil as it came out of the pot, showing this special character of root-development. There were three small clusters of nodules on the roots of each of the three plants, one or two smaller bunches, and here and there a single nodule. But the clusters were much smaller, the total number of nodules was much less, and they were more distributed throughout the soil, in this pot with rich garden soil, than in either of the others, even than in pot 1, without any soil-extract seeding. As the description shows, the root-development was at the same time much greater than in either of the other pots. To this point we shall have to recur again, but it may be remarked in passing, that the greater development of root and root-fibre, and the less development of the root-nodules, in the soil which itself supplied abundance of nitrogenous, as well as of other nutriment, is consistent with the observations of some other experimenters; but it is, on the other hand, inconsistent with the observations and views of others.

Finally in regard to the relative development of root-nodules under the different conditions, the evidence is clear, that there was a greatly enhanced development of them under the influence of the soil-extract seeding; and that, coincidently with this, there was a considerably increased growth of the above-ground parts of the plant.

The distinctly less development of root-nodules in the rich garden soil, than in the sand with soil-extract seeding, as observed in the case of the peas, was, however, not found in that of the lupins, as the following notes on the roots of the lupins grown in 1888, will show.

In pot 5, with the impure yellow sand, but without soil-extract, eventually three plants of blue lupins grew. From the short, thick, main root, many branches proceeded, extending from the top to the bottom of the soil; those plants having the largest above-ground development had also the most root. The branches were fleshy and succulent, and thicker at a distance from the main stem than near it. No nodules were observed on the roots in this pot.

In pot 6, with lupin-soil-extract, but with only two plants, the roots were of the same general character as to branching, extension, fleshiness, and succulence, as those in pot 5. There was, however, one nodule, about the size of a pea, on a root-fibre on one of the two plants.

In pot 7, with the lupin sand itself, there was no plant.

In pot 8, with the garden soil, there were three plants, two of them small ones from more recent sowings than the other, and with much less root development; but there were three or four nodules or swellings on the root-fibres of each plant. The largest and oldest plant showed a very great development of root, extending throughout the soil, round the sides, and along the bottom of the pot. On the main root, which was thick and strong down to about 5 inches, there were two large swellings or nodules about 3 inches from the surface, each of which, unlike the bunches of nodules on the pea roots, appeared externally to be single and solid, but indented. There were, besides, nineteen small swellings on the root-fibres, of the same colour as the root itself, and whether these were nodules or not was not very obvious.

In pot 9, in the yellow sand without soil-extract, there were two plants of yellow lupins. With less above-ground growth, there was also considerably less root development, than under the same soil conditions with the blue lupins (pot 5). As in the case of the blue lupins, there were, however, no nodules.

In pot 10, with lupin-soil-extract seeding, there were two small plants, also with small root-development, but throwing out much fine fibre near the surface, and then slender branches to the bottom of the pot. There were here, again, no nodules.

In pot 11, with the lupin sand, there were two plants, one very much larger than the other. There was a swelling on the thick main root of the smaller plant, but there were no nodules on the rootlets. The larger and older plant developed a dense mass of both fleshy and fine fibrous root. The main root, about 1 inch from the surface, was encased by a large swelling. The roots extended from the top to the bottom of the soil. No nodules were observed on the rootlets, but there was an abundance of root-hairs.

In pot 12, with garden soil, there were two plants. The stout, woody, main root, extended deeper than in the other pots; and there were many branches, extending round the sides and along the bottom of the pot. The larger plant had two swellings on the main root, about $1\frac{1}{2}$ inch from the surface, each of the size of a field bean; also three small nodules on the root-fibres. The smaller plant had one indication of such a swelling on the main root, and twelve nodules on the root-fibres, three the size of a pea, three half as large, and six very small. The larger were 1 or 2 inches from the surface, the others lower, one 6 inches down. There was more fine fibre, but very much less development of root-hairs than in pot 11.

Thus, in the case of both blue and yellow lupins, there were no nodules without soil-extract, and only one with the lupin soil-extract seeding. In the lupin soil itself there was some indication, but in the garden soil there was, with both descriptions of lupin, a much

more marked development, both of swellings on the main roots, and of nodules on the root-fibres. The very meagre development of nodules both with lupin-soil-extract seeding, and in the lupin-soil itself, in 1888, when, as will be seen further on, the result was so different in 1889, suggests the question whether the lupin-sand of 1888 had been too much dried, and so sterilised.

The Analytical Results.

We will now turn to the evidence afforded by analysis, as to the difference in the amount of growth, and especially as to the difference in the amount of nitrogen assimilated, in the peas grown under the different conditions.

The following table shows the percentages of ash, and of nitrogen determined by copper oxide (each calculated on the dry substance), in the stems and leaves together, and in the roots, of the plants in the different pots.

	Per cent. in dry substance.			
	Ash.		Nitrogen.	
	In stems and leaves.	In roots.	In stems and leaves.	In roots.
	Per cent.	Per cent.	Per cent.	Per cent.
Pot 1. Sand, without soil-extract	19·70	28·67	2·904	2·574
Pot 2. Sand, with soil-extract	16·07	36·75	4·900	3·195
Pot 3. Sand, with soil-extract	13·87	23·26	4·006	3·357
Pot 4. Garden soil	9·17	20·44	4·534	2·791

It is remarkable how much lower is the percentage of ash in the dry substance of the more normally-developed plants grown in the garden soil, than in that of those grown in the sand with plant-ash added. There can be no doubt that the amount of soluble mineral matter provided in the quantity of ash used was excessive; and less has been supplied in the experiments of 1889. The percentage of ash in the dry substance of the roots is, however, in all cases high, but doubtless some adherent sand would be included.

The differences in the percentage of nitrogen in the dry substance of the differently grown plants are consistent with the known characters of growth. Thus, the lighter colour, and the comparatively restricted growth, of the plants in pot 1, indicated nitrogen exhaustion; and the

percentage of nitrogen in the dry substance, of both the above-ground and the under-ground produce, is lower than in either of the other cases. It may be further noted, that the roots grown in pots 2 and 3 with the soil-extract, and with so much greater development of nodules than in either pots 1 or 4, at the same time contained a considerably higher percentage of nitrogen in their dry substance.

The next table shows the actual quantities of dry substance, of ash, and of nitrogen, in the separated, and in the total, products of growth.

	Actual amounts in the produce.								
	Dry substance.			Ash.			Nitrogen.		
	In stems and leaves.	In roots.	In whole plant.	In stems and leaves.	In roots.	In whole plant.	In stems and leaves.	In roots.	In whole plant.
	grams.	grams.	grams.	grams.	gram.	grams.	gram.	gram.	gram.
Pot 1	7·423	2·600	10·023	1·462	0·745	2·207	0·2153	0·0669	0·2822
Pot 2	9·368	2·409	11·777	1·505	0·885	2·390	0·4591	0·0770	0·5361
Pot 3	9·411	1·748	11·159	1·305	0·407	1·712	0·3771	0·0587	0·4357
Pot 4	12·808	2·846	15·654	1·175	0·582	1·757	0·5816	0·0794	0·6600

It is seen that there is more dry substance in the above-ground growth, but less remaining in the roots, in pots 2 and 3 with the soil-extract than in pot 1 without it. In the whole plant there is, of dry substance with soil-extract, about $11\frac{3}{4}$ grams in pot 2, and more than 11 grams in pot 3, against only 10 grams in pot 1 without soil-extract.

The point of chief interest is, however, that there was twice, or more than twice, as much nitrogen in the above-ground growth in pots 2 and 3 with the soil-extract seeding, as in pot 1 without it. But there is much less difference in the amount of nitrogen remaining in the roots under the different conditions. In the total vegetable matter there is in pot 2 more than twice, and in pot 3 nearly twice, as much nitrogen as in pot 1 without the soil-extract.

With the full supply of already combined nitrogen in pot 4, with garden soil, there was about one and a third time as much dry substance produced, and more nitrogen assimilated, than under the influence of the soil-extract seeding.

The significance of the results relating to the nitrogen is, however, more clearly brought to view in the next table, which shows—the amounts in the soils at the commencement and at the conclusion of the experiment, and the gain or loss; the amounts in the seed, in the total products of growth, and the gain; the total nitrogen in the

soil and seed at the commencement, in the soil and produce at the conclusion, and the gain. Lastly, in the last column but one, the total gain, reckoning in each case, the initial nitrogen = 1; and, in the last column, the gain in the plants, reckoning the nitrogen in the seed = 1.

[The results relating to the soils in pots 1, 2, and 3, are calculated from the copper oxide determinations; which gave, in the dry sand at the commencement (as already shown) 0·00245 per cent. nitrogen; and at the conclusion the amounts were:—0·00269 in that of pot 1, 0·00239 in that of pot 2, and 0·00208 in that of pot 3. The determinations in the garden soil were by the soda-lime process, and were only made to obtain a general idea of the result, and were not intended for exact quantitative estimates of gain or loss. Thus, the higher the percentage of nitrogen, the smaller the quantity that can be taken for burning, whilst such a soil, rich from the application of dung, is a very heterogeneous mixture, and difficult to sample for analysis. It, moreover, contains a very large amount of carbon, and gives a coloured acid for titration. Nor was the nitric acid determined, either at the commencement or at the conclusion; but, with so much organic matter the error, if any, thus arising would be immaterial. The determinations were, however, fairly accordant for such material; giving, calculated on the dry soil, at the commencement, 0·4341 and 0·4379, mean 0·4360 per cent.; and at the conclusion, 0·4378 and 0·4342, mean 0·4360. It may be added, that a difference or error of 0·01 in the percentage of nitrogen in the soil, would represent a gain or loss of 0·204 gram on the quantity of the garden soil used. The data upon which the amounts of nitrogen in the seed, and in the products of growth, are calculated, have been already considered.—January 24, 1890.]

	Nitrogen.										
	In soil.			In seeds and produce.			Total.			In total products, total initial =1.	In plants, nitrogen in seed = 1.
	At commencement.	At conclusion.	Gain (+) or loss (−).	In seeds sown.	In total plants.	Gain.	At commencement.	At conclusion.	Gain.		
	grams.	grams.	gram.	gram.	gram.	gram.	grams.	grams.	gram.		
Pot 1...	0·0999	0·1096	+0·0097	0·0293	0·2822	0·2529	0·1292	0·3918	0·2626	3·03	9·63
Pot 2...	0·0999	0·0974	−0·0025	0·0298	0·5361	0·5063	0·1297	0·6335	0·5038	4·88	17·99
Pot 3 ..	0·0999	0·0848	−0·0151	0·0291	0·4357	0·4066	0·1290	0·5205	0·3915	4·04	14·97
Pot 4...	7·9989	7·9989	—	0·0301	0·6600	0·6299	8·0290	8·6589	0·6299	1·08	21·93

The first point to notice is, that there is very little difference in the amount of nitrogen in the soils at the commencement, and at the conclusion, of the experiments. There would, doubtless, be some

fine root-fibre not removed at the conclusion, so that where there is loss it is to be supposed that some of the original nitrogen of the soil has contributed to the growth. In the case of the garden soil, with its high percentage of nitrogen, it is of course not impossible that there may have been some loss by evolution of free nitrogen.

That there is at any rate no material gain in the soils would seem to be confirmatory of the conclusion indicated by other evidence, that the fixation of nitrogen is not effected by the organisms within the soil independently of the symbiotic growth of the nodules and their contents and the higher plant to which they are attached, to whose nitrogenous supply they seem to contribute. Indeed, if the fixation had taken place under the influence of microbes within the soil, independently of connexion with the higher plant, we should have to conclude that the latter had, nevertheless, availed itself of exactly the whole of the nitrogen so brought into combination—a supposition for which there would seem no reasonable justification.

Turning to the middle division of the table, which shows the nitrogen in the seed sown, in the total vegetable matter grown, and the gain, and disregarding the changes in the soil itself, which it has been seen may well be done, it will be observed that the gain of nitrogen in the plants is so large as to be very far beyond the limit of any possible experimental error. This certainly cannot be said of some of the experiments conducted on other lines, the results of which have been published in recent years, and been held to show the fixation of free nitrogen under the agency of micro-organisms within the soil, without coincident higher plant-growth, or with the coincident growth of other plants than of the leguminous family.

The gain in these initiative experiments with peas is, however, much less than in many of those of Hellriegel and Wilfarth. This is not to be wondered at, when the late period of the season, and the consequent character of the growth, are borne in mind; and when we come to consider the greater growth attained in the experiments of 1889, little doubt can be entertained that the fixation was then very much greater than it was in 1888.

To refer to the figures, it is seen that, whilst the nitrogen supplied in the seed was only 0·03 gram or less, that of the products of growth was 0·2822 gram in pot 1, 0·5361 in pot 2, 0·4357 in pot 3, and 0·6600 in pot 4; and the gains are $\frac{1}{4}$ of a gram in pot 1, more than $\frac{1}{2}$ a gram in pot 2, nearly $\frac{1}{2}$ a gram in pot 3, and more than $\frac{1}{2}$ a gram in pot 4.

The third division of the table shows—the total nitrogen at the commencement (in soil and seed together), at the conclusion (in soil and total vegetable matter grown), and the gains. But the significance of the results is more clearly seen in the last two columns. The first of these shows the relation of the amount of nitrogen in the total products (soil and plants together) to the total initial nitrogen (soil

and seed together), taken as 1. It is seen that, even in pot 1, with the impure and not sterilised sand, but without soil-extract, there was, so reckoned, more than three times as much nitrogen in the products as in the soil and seed; in pot 2, with soil-extract, there was about five times as much; and in pot 3, also with soil-extract, there was more than four times as much. In the case of pot 4, however, with garden soil, owing to the large amount of initial nitrogen in the soil, the gain, so reckoned, appears but small.

It is in the last column of the table, in which, disregarding the nitrogen in the soils, which remained so nearly constant throughout, and reckoning the relation of the nitrogen in the total products of growth to that in the seed taken as 1, that the large amount of fixation is brought clearly to view. So reckoned, the nitrogen in the substance grown was—in pot 1, 9½ fold; in pot 2, nearly 18 fold; in pot 3, nearly 15 fold; and in pot 4, nearly 22 fold, that supplied in the seed.

The Vegetation Experiments in 1889.

In this second season a more extensive series was arranged. The plants selected were—peas, red clover, vetches, blue lupins, yellow lupins, and lucerne. For the lupins and lucerne, specially made pots of glazed earthenware, about 6 inches in diameter, and 15 inches deep inside, that is about twice as deep as the pots used in 1888, and as used again for the peas, red clover, and vetches, were employed. These pots had holes at the bottom for drainage, and slits at the side, near the bottom, for aëration. All the pots stood in specially made saucers or pans of the same material. A quantity of broken, washed, and this time ignited flint, was put into the bottom of each pot. The sand used was a rather coarse white quartz sand, from which the coarser and the finer portions were removed by sifting, and more of the finer by washing and decantation, first with well, and afterwards with distilled water. In defect of means for igniting so large a quantity of material (about 300 lbs.) without running the risk of gaining more impurity than was expelled, the portion retained for use was kept, in successive lots, in a large water-bath, at nearly 100° C., for several days, and then preserved in well-closed bottles. The results will show that the sand so prepared was sufficiently, if not absolutely, sterilised.

In each case the sand was mixed with 0·1 per cent. of the plant-ash, and 0·1 per cent. of calcium carbonate.

There were four pots of each description of plant. Of the peas, clover, vetches, and lucerne, No. 1 was with the prepared quartz sand without soil-extract; No. 2 with the quartz sand and garden soil extract added; No. 3 was duplicate of No. 2; and No. 4 was with the garden soil itself. Of the blue and yellow lupins, No. 1 was with

the prepared quartz sand without soil-extract; No. 2 with lupin-soil-extract added; No. 3 was duplicate of No. 2; and No. 4 was with the lupin soil itself, to which 0·01 per cent. of the plant-ash was added.

The soil-extracts were in all cases added on July 9, before the sowing of the seed; 25 c.c. in the case of the peas, vetches, and clover, and 50 c.c. in that of the lupins and lucerne.

The seeds, carefully selected and weighed as in 1888, were sown on July 10, that is, about four weeks earlier than in the previous year, but still not so early as was desirable. In the case of the clover, ten seeds were sown in each pot; in that of the blue and yellow lupins three, and in that of the peas, vetches, and lucerne, only two seeds were put in each pot.

In all four pots, the peas germinated and grew well from the beginning. In the No. 1 pot of vetches, one seed failed and had to be replaced. Several of the red clover seeds failed, and eventually four plants only were left in each pot. As in 1888, most of the blue lupins failed; and eventually only one plant in only one of the four pots, remained. Some of both the yellow lupins and the lucerne also failed; but, as will be seen further on, eventually two good plants of each remained in each pot.

No analytical details relating to the experiments of 1889 are yet available; but the notes on growth, and the photographs of the plants and of their roots convey a clear idea of the importance and significance of the results obtained.

The peas were taken up on October 23 and 24. Photographs of the four pots of plants were taken on August 3, August 20, September 27, and October 22, that is the day before taking up; and an enlargement of the last taken is exhibited. It is seen that, unlike the result obtained in pot 1 in 1888 with the impure and non-sterilised sand, the plants in the purer and sterilised quartz sand, show extremely limited growth. Before the end of July, the plants in both pots 2 and 3, with soil-extract, began to show enhanced growth compared with that in pot 1, without soil-extract seeding; and eventually, whilst the plants in pot 1 were only $8\frac{1}{4}$ and $8\frac{1}{2}$ inches in height, those in pot 2 with soil-extract were 14 and $50\frac{1}{2}$ inches; and those in pot 3, also with soil-extract, were $52\frac{1}{2}$ and $50\frac{1}{2}$ inches high. In pot 4, with the garden soil, the plants showed even somewhat less extended growth than those in pots 2 and 3 with the soil-extract only. But the plants in pot 4 were more vigorous, and whilst they flowered and seeded, neither of those in either pot 2 or 3 did so; but continued to vegetate, the upper parts apparently at the expense of the lower.

The root development should be briefly noticed. In pot 1, without soil-extract, it was altogether much less than in either pot 2 or pot 3 with soil-extract, or than in pot 4 with garden soil. Enlarged photo-

graphs of the roots of pots 1, 3, and 4, clearly illustrate this. It is further seen that, in pot 1, without soil-extract, the main roots descended some distance before they threw out any considerable amount of root-branches and of root-fibre; whereas, in pots 2 and 3, with soil-extract, there was characteristically much more fibre distributed both in the upper layers and throughout the pot.

It is specially to be noted that, whereas in pot 1 in 1888, with impure and non-sterilised sand, there was a considerable development of nodules, now in the pure and sterilised sand, not a nodule was observable.

In pot 2, with soil-extract, one plant was very much larger than the other, and developed very much more root. The smaller plant had, however, several nodules on the main root, near the surface of the soil, and a good many small ones distributed along the fibres. Most of the nodules were more or less shrivelled. The larger plant had a large cluster of nodules on the main root, very near the surface; and a very large number of single nodules, mostly small, was distributed on the root-fibres, quite to the bottom of the pot. Upon the whole those on the larger plant were less shrivelled.

In pot 3, also with soil-extract, the main roots extended to, and along, the bottom of the pot; throwing off many side branches, with a very large quantity of fine fibrous root. The greatest distribution was, however, in the upper few inches of the soil. There were two clusters of nodules on one of the plants, and three on the other, besides smaller bunches. A large number of mostly single small nodules was also distributed along the roots. On one of the plants, the largest cluster was on the main root, and on the other the clusters were on the side branches.

In pot 4, with the garden soil, there was a dense mass of root-fibre throughout the first 6 inches of depth. There were numerous nodules, the majority single, and within the upper 2 or 3 inches of soil. There were also a few small bunches.

Thus, then, the limited growth in pot 1, without soil-extract, is coincident with the entire absence of nodule-formation; and the increased growth in pots 2 and 3, with soil-extract, is coincident with a very great development of nodules. In pot 4, with garden soil, itself supplying abundance of nitrogen, there was also a considerable development of nodules, but distinctly less than in pots 2 and 3, with soil extract only.

The vetches were taken up on October 26. They had been photographed on August 3, August 20, September 27, and lastly on October 25, that is, the day before taking up; and of this last representation an enlargement was exhibited.

Here, as with the peas, the plants in pots 2 and 3, with soil-extract, had shown more growth than those in pot 1 without it, before

the end of July. Again, as with the peas, the vetches in the pure and sterilised sand showed extremely limited growth. On the other hand, those in pots 2 and 3, with the soil-extract grew, as shown in the photograph, to a very great height; indeed, higher than those in pot 4 with the garden soil.

The heights of the plants were—in pot 1, without soil-extract, $11\frac{1}{4}$ and $10\frac{1}{2}$ inches; in pot 2, with soil-extract, $52\frac{1}{2}$ and 67 inches; in pot 3, also with soil-extract, $61\frac{1}{2}$ and 51 inches; and in pot 4, with garden soil, only 53 and 36 inches.

But, as in the case of the peas, whilst the plants in pot 4 with the garden soil flowered and seeded, those in pots 2 and 3, with the soil-extract only, did not, but continued to extend upwards at the expense of the lower parts of the plant.

There was much less development of root in pot 1, without soil-extract, than in either pots 2 or 3 with it, or than in pot 4 with the garden soil. The main roots descended to the bottom of the pot, and threw out a number of side branches, but there was a marked deficiency of root-fibre. Not a single nodule was found.

In pot 2, with soil-extract, there was, as shown in a photograph, a dense mass of root and root-fibre, which distributed throughout the whole of the soil, though the greatest accumulation was within the first 3 inches of depth. There were numerous nodules, but considerably less in quantity than on the corresponding pea-plants. They were mostly single, the greater number being found in the lower layers, which is also contrary to the result with the peas. They were, moreover, generally exceedingly small.

In pot 3, also with soil-extract, there was also an immense development of root and root-fibre through the whole area of the soil; the greatest accumulation being in the upper and lower portions of the pot, with less in the middle. There were many nodules, but very small, and probably fewer than on the roots in pot 2. All the nodules were single, and fairly distributed over the whole root area.

In pot 4, with garden soil, there was a moderate amount of root and of root-fibre, chiefly within the upper 6 inches of depth; but there was altogether very much less of root development than in either pots 2 or 3 with the soil-extract. There were many nodules, but all single, and very small; and they appeared to be flattened, as if exhausted of their contents.

Here again, then, as with the peas, the very restricted growth in pot 1, without soil-extract seeding, was associated with very limited root development, and with the entire absence of nodule-formation. On the other hand, the very greatly extended vegetative growth in pots 2 and 3, with soil-extract, was associated with an immense development of root and root-fibre, extending throughout the pots, and with the formation of numerous nodules; which, however, were generally

smaller, more distributed over the whole root area, and less accumulated near the surface, than in the case of the peas. Lastly, in the garden soil, with its liberal supply of combined nitrogen, there was much less development of roots, and less also of nodules, than in the pots with soil-extract only.

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It has already been said that most of the blue lupins failed; but it was with the yellow lupins that the most striking results were obtained.

As in the case of the other plants, the yellow lupin seeds were put in on July 10, three being sown in each pot. There were some re-sowings, some seeds taken out, and, eventually, two plants were left in each pot. By the end of July those in pots 2 and 3, with the lupin-soil-extract seeding, already showed more growth than those in pot 1 without it. Photographs were taken on August 3, August 20, September 27, October 28, and November 29; and the plants were cut on December 7. An enlargement of the photograph taken on October 28 was exhibited; and the later representation, that of November 29, was thrown on the screen.

It is seen that the plants in pot 1, without soil-extract seeding, scarcely appeared over the rim of the pot, one being only about $1\frac{1}{2}$, and the other about $2\frac{1}{2}$ inches high. In pot 2, with lupin-soil-extract seeding, one plant was about 2 feet, and the other more than $1\frac{1}{2}$ foot high; both spreading much beyond the width of the pot. In pot 3, also with lupin-soil-extract seeding, one plant was more than 2 feet, but the other little more than 8 inches high. In fact, in both these pots with soil-extract seeding only, the plants showed considerably more development than those in pot 4 in the lupin-soil itself; one of these being only about 16, and the other about 18 inches high, and both less branching than those in pots 2 and 3.

Unlike the peas and vetches, the yellow lupins with soil-extract seeding flowered and podded freely. One plant in pot 2 had nine small pods; and one in pot 3, four large and three small ones. There were also in pot 4, with lupin-soil, on one plant five pods, and on the other six.

Thus, in the quartz-sand with lupin-soil-extract seeding, the plants not only produced a great deal more vegetable matter than those in the lupin-sand itself, but they as freely flowered and seeded.

Photographs of the roots of the plants in each of the four pots were taken; and enlargements of those in pot 1 without soil-extract seeding, in pot 3 with soil-extract seeding, and in pot 4 with the lupin-soil itself, were exhibited.

In pot 1, without soil-extract, and very restricted above-ground growth, there was coincidently very little root development. The main roots descended far down the deep pot almost without branching, but at the bottom a number of branches, and a mass of fibre were produced. The root-fibres were fleshy and succulent. No root swellings or nodules were found.

In pot 2, with the lupin-soil-extract seeding, there was, on the other hand, a very great development of root. Branches were thrown out throughout the whole length; and at their ends masses of fleshy fibrils were formed, which were thickly coated with root-hairs. On the main root of one plant, 3 inches down, there was a large swelling or nodule the size of a field bean; 4 inches lower there were three smaller ones on a side branch; 10 inches down there were three as large as peas; and lower still there was another small swelling, more like the nodules found on other plants. The other plant had less root growth. One and a half inch down there was a swelling the size of a small pea; and $4\frac{1}{2}$ inches lower there were three swellings, one as large as a bean, and the others about the size of a vetch seed. These swellings on the lupin-roots, which were all on the main roots or thicker branches, are very different in appearance from the nodules on the pea and vetch-roots. They are, as described, swellings, encasing the root where they grow.

In pot 3, also with the soil-extract seeding, one plant, as an enlarged photograph shows, developed an immense amount of branching root, with a great deal of root-fibre, which extended throughout the whole soil, but to a greater degree in the lower than in the upper half of the pot. The main root was woody near the top. The lower root-fibres were fleshy, and thickly coated with root-hairs. There were several swellings or nodules on the main root below 5 inches; and lower down, on a root-branch, there were several swellings; there being in all twelve on this plant. On the smaller and more meagrely rooted plant, about 10 inches down, there were also two bunches of small nodules, and three single nodules; and a little lower, on a side branch, another small nodule. With regard to the great development of root-hairs on the fine fibrils of the roots in both pots 2 and 3, with quartz sand and soil-extract seeding, it may be supposed that this was an effort to acquire mineral nutriment, in quantity commensurate with the large amount of nitrogen fixed, and available to the plant.

In pot 4, with the lupin-sand, the distribution of root was very different from that in pots 2 and 3, with the soil-extract. The main root, at a depth of 2 inches, threw out many thread-like branches, at the end of each of which there was a bundle of fine fibre. The lower fibres became thicker, and were white and fleshy; but they were without the marked development of root-hairs observed in such

abundance in pots 2 and 3. Most of the root was within 6 inches of the surface, and there seemed to be none below 14 inches. One to two inches from the surface, there were swellings on the main roots which were less raised, but more spreading, than those on the roots in pots 2 and 3. There were also, on one side branch, six very small nodules.

To sum up in regard to the yellow lupins: Under the influence of the soil-extract seeding, the above-ground growth was not only very luxuriant, but the plants developed great maturing tendency, flowering and seeding freely. The development of the roots generally, and that of swellings or nodules on them, were also very marked; and there can be no doubt that the gain of nitrogen will be found to be very large. In pot 4, with the lupin-sand itself, which would supply a not immaterial amount of combined nitrogen, although the growth was normal, it was, both above ground and within the soil, very much less than in the pots with soil-extract only; and the development of nodules was also less. It is possible that the less development in the lupin-sand itself, than in the quartz-sand with soil-extract only, was partly due to the much less porosity of the lupin-soil, especially when watered. At any rate, the results with the soil-extract only are very remarkable.

As the main growth of red clover is in the second year, and that of lucerne also in years subsequent to the first, the pots of these plants are left for further growth; so that there is, at present, but little of definite result available in regard to them. There are, however, some points of special interest to notice.

A photograph of the clover plants taken on September 28 was thrown on the screen. The above-ground growth in pot 1, without soil-extract, was distinctly more than in either pots 2 or 3 with it; and it is judged that the amount of growth will probably prove to be greater than is to be accounted for by the amount of nitrogen supplied in the seed sown. As the soil-extract seeding in pots 2 and 3 seemed to be without effect, a second amount of extract, but this time from garden soil where clover was growing well, was, on September 4, applied to pot 2; but to pot 3 there was added instead a solution of calcium nitrate, and this application was continued up to December 6, when, in all, 0·23 gram of nitrogen had been so applied. The effect of the nitrate was, undoubtedly, some increased growth, but especially an increased depth of green colour. It remains to be seen what will be the final result.

The application of garden-soil-extract to lucerne also appeared to be entirely without effect up to the beginning of September; the plants in pot 1 without soil-extract, and those in pots 2 and 3 with it, showing no difference, and apparently no progress. On September 4, therefore, pot 2 was re-seeded with soil-extract, this time from a soil

growing lucerne; and, at the same time, a solution of calcium nitrate was added to pot 3, and the application was continued, as in the case of the clover. For many weeks the repeated soil-extract seeding was without any apparent effect; but, quite recently, there has been a slightly increased depth of colour, and perhaps a little growth. The application of nitrate to pot 3, however, showed marked effect very soon after the application had been commenced; and, as the representation of the growth on December 23 shows, there was up to that time, considerable growth under the influence of the nitrate.

The darkening of the colour of the leaves of the clover, and the increased growth of the lucerne, under the influence of the nitrate, in soil otherwise nitrogen-free, is of interest. Not that there is any want of abundant evidence showing that Leguminosæ do take up nitrogen largely as nitrate, but, in view of the new results under the influence of micro-organism seeding, it seems to be assumed by some that these plants probably depend for their nitrogen exclusively on such agency.

Before concluding in regard to the experimental plants, some reference should be made to the very great difference in the external appearance and character of the swellings, or nodules, on the roots of the different descriptions of plant, and even on those of the same description. In the course of the examinations this was so marked, that it was contemplated to take photographs illustrating the most characteristic differences; and, as it was found that the roots of the experimental plants, which had to be preserved for analysis, could not without risk be manipulated as required for the purpose, some plants were procured from the garden and the fields, and notes of previous observations were looked up. Presumably owing to the late period of the season, the roots so obtained were, however, not suitable for the illustration desired. It must suffice, therefore, avoiding any attempt at technical description, to make a few general observations on the facts at command; and to say that we hope to follow the subject up at a more suitable season of the year, and then to be able to give some account, not only of the general external, but, if possible, of the internal characters of the different bodies.

It should be stated that, so far as the nodules on the roots of the bean are concerned, a full technical description, both of their external characters and internal structure, has been given by Professor Marshall Ward ('Phil. Trans.,' B, 1887, vol. 178, p. 539, *et seq.*).

Reference to the descriptions which we have already given will show, that the external appearance, and distribution, of the nodules was very different on the roots of the peas, the vetches, and the lupins. In the case of the peas there were many of what may be called agglomerations of nodules, and comparatively few single ones distributed on the root-fibres. On the roots of the vetches, there

were comparatively few agglomerations or bunches, and more single nodules, pretty widely distributed along the root-fibres. The lupin roots, on the other hand, showed tubercular developments very different from those on either the pea or the vetch roots. Indeed, at the period of examination, that is when the plants were nearly ripe, two apparently distinct kinds were observed; one of which, the most prevalent, we have spoken of as "swellings," and the other as "nodules." The "swellings" were chiefly on the main roots or the thicker branches; where they grew they encased the root entirely, and they had a shining and presumably impervious skin. The "nodules," on the other hand, were chiefly single, small, and distributed on the root-fibres. Assuming that the so-called "swellings" were the bodies which, with their contents, had exercised the functions of the "nodules" found on the roots of the other plants, it is to be concluded that, after the very luxuriant growth, and the flowering and seeding, their function was so far at an end, and they had become suberised. The other bodies on the lupin roots, distinguished in the description as "nodules," indicated too meagre development to have had much share in the great amount of assimilation that had been accomplished. On the other hand, the "swellings," as has been said, were all on the main roots or thicker branches; whilst it is generally stated that the nodules are only formed on the young and still growing roots. If these "swellings," which were certainly very characteristic of the roots of the plants which attained the greatest growth, were really the effective nodules, it must be supposed that they had been formed where they were found, whilst the root was still young, and had grown with its growth. In favour of this supposition is the fact that the increased growth from the soil-extract seeding commenced quite early in the life of the plants.

In 1887, the nodule development on lucerne roots was observed at different periods of the season, and again quite recently, on plants taken from the field for that purpose. The nodules on the roots of lucerne are quite different in general external character from those on any of the other plants that have been examined at Rothamsted. Instead of being more or less rounded, they have more the appearance of shoots or buds, much longer than broad, sometimes single, but more often divided, or branched; there being generally two or three, and sometimes as many as twenty, or even many more, in a bunch, joined at the base. They have not been observed on the main root, but only on the root-fibres, and less near the surface than within the range of the clay subsoil. In some cases such a tuft or bunch will be at the end of a fine fibre by which it is connected with the main root. As the season advances these bodies become shrivelled, and are in fact empty shells. The question arises, whether in the case of the development in soil or subsoil containing organic nitrogen,

the lower organisms may not serve the higher, in part at least, by taking up, either directly or indirectly, combined nitrogen; as, for example, fungi take up organic nitrogen from the soil; or as, it may be assumed, does the fungus in the case of the fungus-mantle observed by Frank on the roots of Cupuliferæ, and some other plants?

Among the Leguminosæ growing in the mixed herbage of grass land, in 1868, nodules were observed on the root-fibres—of *Lathyrus pratensis*, especially near the surface of the soil; on the ultimate root-fibres of *Trifolium pratense*, and on the smaller rootlets of *Trifolium repens*.

In the case of red clover growing in rotation on arable land, an abundance of nodules has been found, both near the surface and at a considerable depth. They are generally more or less globular or oval. Some found on the main roots are more like "swellings" than attached tubercles, not, however, encasing the root, but only on one side. The greater number are, however, small, and distributed chiefly on the root-fibres. Observations are, however, needed, as to any difference in character, or rélative prevalence, at different periods in the life and growth of the plant, and under different conditions of soil, both so far as mechanical state and porosity, and richness or otherwise in available supplies of combined nitrogen, are concerned. To these points we hope to pay some attention.

Referring to the main object of the investigation, it will be admitted that the results so far brought forward are abundantly confirmatory of those obtained by Hellriegel, and that the fact of the fixation of free nitrogen in the growth of Leguminosæ, under the influence of microbe seeding of the soil, and of the resulting nodule formation on the roots, may be considered as fully established.

It appears that, almost concurrently with the experiments made at Rothamsted, M. Bréal, of the Physiological Laboratory of the Muséum d'Histoire Naturelle, of Paris, has made various experiments on lines suggested by the results obtained by Hellriegel and Wilfarth. He examined the contents of nodules from lucerne roots, and observed rounded grains and bacteria-like filaments. He determined the nitrogen in the root-tubercles from various Papilionaceæ, and found it much higher in them than in the stalks, leaves, or roots. He germinated peas in a nutritive solution, and added some of the matter from a crushed lucerne root-tubercle. The pea roots became covered with tubercles, and eventually the nitrogen in the plant was about double that in the seed sown. In another experiment he germinated two lupin seeds, inoculated one of them from a living lucerne root-tubercle, and planted both in gravel with a nutritive solution free from nitrogen. Eventually the roots of the inoculated plant were covered with tubercles, whilst those of the other had

none. The inoculated plant also contained about two and a half times as much nitrogen as the seed, whilst without inoculation there was practically no gain. This experiment has been repeated by Hellriegel with very striking results, as one of us had the opportunity of seeing in August last. In another experiment, peas were germinated in a lucerne soil, transplanted into gravel, and nutritive solution free from nitrogen added, when the roots became covered with tubercles, and the nitrogen assimilated was nearly twenty-five fold that of the seed. On inoculating the germinated roots of haricots, and planting them in sand, they grew vigorously, formed pods, developed many tubercles on their roots, and assimilated nearly fifteen times as much nitrogen as the seed supplied. Lastly, he planted a fragment of lucerne root with nodules on it, in a sandy soil, reserving a similar fragment for analysis. Several cuttings of lucerne were obtained; and when taken up the root had many nodules, and the nitrogen assimilated was more than eighty times as much as in the root planted.

As to the importance to agriculture, in a quantitative sense, of this newly established source of nitrogen to the Leguminosæ, the evidence at present at command is insufficient to enable us to form any very decided opinion. Both agricultural investigation and direct vegetation experiment have clearly shown that Leguminosæ do take up much soil-nitrogen, and, at any rate in great part, as nitrate. But in our recent paper in the 'Philosophical Transactions' before referred to, we showed that, in some special cases, there was no evidence to justify the conclusion that the whole of the nitrogen had been so derived; and it was admitted that some other explanation of the large amounts of nitrogen assimilated was needed. It is not improbable that, in those cases, the agency now under consideration contributed to the result.

Then, as to the growth of leguminous crops in the ordinary course of agriculture. Hellriegel agrees with us that they do utilise soil-nitrogen, and he thinks probably always first; but that that source is supplemented by nitrogen brought into combination under the influence of the symbiotic growth of special organisms and the higher plant; and he supposes that the proportion of the total nitrogen assimilated which will be due to this latter source will be greater in crops grown in soils that are poor than in those which are rich in nitrogen. He considers it probable, however, that even in the case of rich soils there will be always more or less gain due to such fixation. The proportion of the nitrogen assimilated which will be gain depends, therefore, on complicated conditions. As bearing upon this subject it may be stated that, in experiments with beans, Professor Vines found that the formation of tubercles on the roots was very much reduced, if not indeed only accidental, when the plant was liberally supplied

with nitrate. Again, certainly the evidence of the experiments which have been described, so far as it goes, seems to indicate a less development of nodules when the soil contained an abundance of combined nitrogen. If this indication should be confirmed, and the inference be generally applicable, it would be concluded that the agency of the symbiotic growth supposed, in fixing free nitrogen, will, other things being equal, be the less the more the soil itself is in a condition to supply an abundance of combined nitrogen; whilst its capability in this respect will depend not only on the richness in combined nitrogen of the soil within the range of the roots, but on its state of combination, and on the character of the soil as to porosity and aëration. On the other hand, the development of the supposed nitrogen-fixing organisms obviously depends on the infection of the soil with the organism essential to symbiotic life with the particular leguminous crop to be grown. It would also seem that it is, at any rate in some cases, dependent on the due porosity and aëration of the soil.

Should these assumptions be borne out by the results of future investigation, we may conclude that the proportions in which any particular leguminous crop will derive its nitrogen from soil-supplies of combined nitrogen on the one hand, and from fixation under the influence of the symbiotic growth on the other, will be very different, according to the characters of the soil, as to available supply of combined nitrogen, mechanical condition, and due infection. We should further conclude that, in such cases as those in which poor sandy soils will not grow fair crops of cereals, but will nevertheless yield enormous crops of some leguminous plant—lupins, for example—the leguminous crop will depend for a large proportion of its nitrogen on fixation, under the influence of the symbiotic growth. Again, in such cases as those of the growth of lucerne for many years in succession, as in some Continental countries, it may be supposed that such fixation would be the source of a considerable proportion of the very large amounts of nitrogen assimilated over a given area under such conditions.

In the case of beans, there is evidence that there is nodule-formation when the plant is grown under ordinary conditions, in the garden or in the field; it has also been seen that nodules were formed on the roots of the peas and the vetches experimentally grown in garden-soil; and the inference so far is, that wherever there is such formation, there is more or less fixation.

Then, as to the important case of the growth of red clover in our rotations. There can be no doubt that red clover does avail itself of soil supplies of combined nitrogen. On the other hand, the so-called leguminous nodules have frequently been observed on the roots of red clover growing in the field. Further, although Hellriegel in his earlier experiments did not get definite results with clover, he has

subsequently obtained increased growth by seeding with extract from both a loamy humus-soil, and a root-crop-soil; but the result was less marked than with some other Leguminosæ. It has been seen that, in the first year of the experimental growth of clover at Rothamsted, no beneficial effect resulted from seeding with rich garden-soil-extract. It is believed, however, that the growth in the sterilised sand without soil-extract seeding will prove to be greater than can be accounted for by the supply of nitrogen in the seed sown. If this should turn out to be the case, the supposition will be that the necessary infection has come from the atmosphere. In reference to this point it may be mentioned that the glass-house in which the experiments are conducted stands in the midst of allotment gardens, in which a great variety of vegetables is growing, whilst Hellriegel's most definite result with clover was obtained by seeding with an extract from a root-crop soil.

Existing evidence is, therefore, in favour of the supposition that red clover does derive some of its nitrogen from fixation under the influence of proper soil-infection, and the resulting symbiosis of the lower and the higher growth. There is, however, at present very little definite evidence to guide us in judging under what conditions, on the one hand soil supplies of combined nitrogen, and, on the other such fixation, will contribute more or less of the total nitrogen of the crop. As one important element in forming a judgment on the subject, it is, as already said, our intention to study the conditions under which the development of nodules on the roots of growing clover is more or less favoured.

Upon the whole, then, the evidence at command points to the conclusion that, in the case of most if not all of our leguminous crops, a greater or less proportion of their nitrogen will be due to the fixation supposed.

Admitting the fact of such fixation to be fully established, the question still remains, how is it to be explained? Unfortunately, here again, as in the matter of the importance to agriculture in a quantitative sense, of this source of nitrogen to our crops, there is much yet to learn before a satisfactory answer can be given. Hellriegel frankly admits that a satisfactory explanation is still wanting; and we agree with him that we must know more of the nature and mode of life of the organisms which, in symbiosis with the leguminous plant, bring about the fixation of free nitrogen, before the nature of the action can be understood. As to the mode of life of these bodies, we owe much to the investigations of Marshall Ward, Prażmowski, Beyerinck, and others; but probably none will more readily admit than themselves, that the facts which they have established so far, are insufficient to afford an adequate explanation of the phenomena involved.

It is, it seems to us, a point of importance that it should be established, as it appears clearly to be, that in the development of the parasite the cortex of the host is penetrated, and so an intimate connexion between the two, indeed a symbiosis, is set up. Then there is abundant evidence that the nodules are very rich in nitrogen. So far as the facts at command go, it would seem that their dry substance may contain a higher percentage of nitrogen than that of any other part of the still growing plant; and, in some cases at any rate, even higher than in that of the highly nitrogenous leguminous seed itself.

Whence comes this nitrogen? The opinions of those who have specially studied the histology and biology of the subject, do not seem to be very clear or definite in reference to this point. According to Prażmowski, as quoted by Marshall Ward, the bacteroids "can only multiply in the still *living* protoplasm." Again, under the influence of the fungus—"the young tubercle is developed in the deeper parts of the cortex, and in its tissues the bacterium-like contents of the fungus become distributed, and grow, divide, and branch at the expense of the protoplasmic contents. He regarded the phenomenon as one of symbiosis, and as benefiting the host as well as the parasite." And again—"The tubercle-bacteria penetrate through young (not suberised) cell membranes into the root-hairs and epidermis cells of the root, and there multiply at the expense of the protoplasmic cell-contents."

Further, "The contents of the bacteroid cells are resorbed as the bacteroids dissolve, certain substances being left behind. In other words, the plant utilises the substance of the bacteria. When emptying begins, and with what energy it proceeds, depend especially on the quantity of nitrogenous compounds at the disposal of the roots. In a soil rich in nitrogen the tubercles go on developing unhindered, become large and typical, and rosy inside, and are not exhausted till late; in poorer soils they attain no great size, are soon emptied, and are green-grey inside."

Summarising the results and conclusions of Prażmowski, Marshall Ward says—

"No decision is arrived at as to whether the nitrogen is got from nitrogen compounds or from the free nitrogen of the air, nor as to what advantage accrues to the bacteria and the host-plant respectively." And again:—

"From the preceding, we see that the tubercles depend on a symbiosis which is advantageous to both the plant and the bacteria. The latter feed on the sap and cell-contents, and multiply through innumerable generations, and, both during the life of the host and afterwards, become redistributed in the soil. The plant derives advantage in that it obtains nitrogen by means of the bacteria.

Though the symbiosis is useful to both, the plant gains most, for it is the more powerful, and sooner or later overcomes the bacteria, to the multiplication of which it sets limits and finally absorbs the substance of the latter. Being the stronger, the plant directs the symbiosis."

If we understand the foregoing statements rightly, it is assumed that the bacteria acquire their nutriment, including their nitrogen, from the protoplasmic cell-contents of the higher plant; and that, on the other hand, the contents of the bacteroid cells are resorbed. "In other words, the plant utilises the substance of the bacteria." But it is obvious that, so far as the nitrogen of the bacteria is derived from the plant itself, the latter is not a gainer in a quantitative sense.

It is further assumed, that the activity of the process depends—"on the quantity of nitrogenous compounds at the disposal of the roots. In a soil rich in nitrogen the tubercles go on developing unhindered, become large and typical, ... in poorer soils they attain no great size, &c." Here, then, combined nitrogen in the soil is supposed to be the source of the nitrogen of the bacteria, and that they develop the more, the greater the supply of it. Undoubtedly, however, the nodules may develop very plentifully in a nitrogen-free soil, and there may be great gain of nitrogen, if only the soil be suitably infected. Indeed, the tendency of the evidence so far at command seems to show, that both the development of the nodules, and the gain of nitrogen, may be the greater in the poorer, but properly infected soil. Further, so far as the combined nitrogen of the soil is the source of the nitrogen there is no gain of it.

Marshall Ward says, however, that no decision is arrived at as to whether the nitrogen is got from nitrogen compounds or from the free nitrogen of the air, nor as to what advantage accrues to the bacteria and the host-plant respectively. But he adds that the symbiosis is advantageous to both the plant and the bacteria; the latter feeding on the sap and the cell-contents, whilst the plant obtains nitrogen by means of the bacteria.

It is obvious, however, that if the nitrogen of the bacteria is derived from the plant itself, it will be quantitatively no gainer by resorbing it. Nor would there be any such actual gain of nitrogen as there undoubtedly is, if the source of the nitrogen, either of the parasite or of the host, were essentially the supplies of combined nitrogen within the soil.

The most probable alternatives seem to be—1. That, somehow or other, the plant itself is enabled, under the conditions of the symbiotic life, to fix the free nitrogen of the atmosphere by its leaves; a supposition in favour of which there seems no evidence whatever. 2. That the parasite utilises and fixes the free nitrogen, and that the nitrogenous compounds formed are taken up by the host. On such a supposi-

tion, the actually ascertained large gain of nitrogen by the leguminous plant growing in a nitrogen-free, but properly infected, soil becomes intelligible. It is admitted, however, that further investigation of the mode of life of the parasite, especially having regard to its surrounding media, is needed.

It seems to us that there is nothing in the evidence pointing to the conclusion that the fixation is effected by the lower organisms within the soil independently of the symbiotic life. We do not here enter into the question, so much discussed of late, as to whether or not there is fixation within the soil under the influence of other low organisms, independently of the associated growth of a higher plant.

In our recent paper in the 'Philosophical Transactions,' before referred to, we said that whilst experience, whether practical or experimental, did not point to an unsolved problem in the matter of the sources of the nitrogen of the agricultural plants of other families, it was far otherwise so far as those of the Papilionaceæ were concerned. Further, that since the question of the sources of the nitrogen of the Leguminosæ had been the subject of experiment and of controversy for about half a century, and it was admitted that all the evidence that had been acquired on lines of inquiry previously followed had failed to solve the problem conclusively, it should not excite surprise that new light should come from a new line of inquiry; and, that hence should be recognised the importance of the cumulative evidence of the last few years, of which that furnished by the experiments of Hellriegel and Wilfarth was certainly the most definite and the most striking, pointing to the conclusion that although chlorophyllous plants might not directly utilise the free nitrogen of the air, some of them, at any rate, may acquire nitrogen brought into combination under the influence of lower organisms, the development of which was, apparently, in some cases a coincident of the growth of the higher plant whose nutrition they were to serve. It was added, that as such a conclusion was of fundamental and far-reaching importance, it was desirable it should be confirmed by independent investigation.

The results even so far obtained, and recorded in this paper, can leave no doubt that this important conclusion is confirmed, so far as a number of agricultural plants of the leguminous family are concerned. The question suggests itself, whether such, or allied agency, comes into play in the nitrogen assimilation of leguminous plants generally, or of that of other than the agricultural representatives of the non-leguminous families to which we owe such plants, or of those of the numerous and varied other families of the vegetable kingdom.

It is true that the families which contribute staple agricultural plants are but few, and that the agricultural representatives of those

families are also comparatively few. The families so contributing are, however, among the most important and widely distributed in the vegetable kingdom; as also are some of the plants they contribute. As prominent examples may be mentioned, the *Gramineæ*, affording the cereal grains, a large proportion of the mixed herbage of grass-land, and other products; also the *Leguminosæ*, yielding pulse crops, many useful herbage plants, and numerous other products. As we have said, there does not seem to be an unsolved problem as to the sources of the nitrogen of other of our agricultural plants than those of the leguminous family. Obviously, however, it would be unsafe to generalise in regard to individual families as a whole, from results relating to a limited number of examples supplied by their agricultural representatives alone. Still, there is nothing in the evidence at present at command, to point to the supposition that there is any fundamental difference in the source of the nitrogen of different members of the same family, such as is clearly indicated between the representatives of the leguminous, and of the other families, supplying staple agricultural products. On the other hand, existing evidence does not afford any means of judging whether or not similar, or allied agencies to those now under consideration, or even quite different ones, may come into play in the nitrogen assimilation of the members of other families which contribute such a vast variety of vegetation to the earth's surface.

We have pleasure in stating that the conduct of the investigation has largely devolved upon Dr. N. H. J. Miller. He has been almost wholly responsible for the analytical work, as well as for the photographing, by which a permanent record, not only of the aboveground growth, but of the root-development of the experimental plants has been secured. It should be added, that Mr. J. J. Willis has materially assisted in the observation and noting on growth; also in the separation of the roots, mounting them for observation and for photographing, and in noting upon them.

II. "On Electric Discharge between Electrodes at different Temperatures in Air and in High Vacua." By J. A. FLEMING, M.A., D.Sc., Professor of Electrical Engineering in University College, London. Communicated by Professor G. C. FOSTER, F.R.S. Received December 16, 1889.

(Preliminary Notice.)

It has been known for some time that if a platinum plate or wire is sealed through the glass bulb of an ordinary carbon filament incandescent lamp, this metallic plate being quite out of contact with

the carbon conductor, a sensitive galvanometer connected between this insulated metal plate enclosed in the vacuum and the external *positive* electrode of the lamp indicates a current of some milliampères passing through it when the lamp is set in action, but the same instrument when connected between the *negative* electrode of the lamp and the insulated metal plate indicates no sensible current. This phenomenon in carbon incandescence lamps was first observed by Mr. Edison, in 1884, and further examined by Mr. W. H. Preece, in 1885.* The primary object of the experiments described in this paper was the further examination of this effect, but the inquiry has extended itself beyond this range and embraced some general phenomena of electric discharge between electrodes at unequal temperatures, and in particular has revealed some curious effects in the behaviour of an electric arc taken between carbon poles towards a third insulated carbon on metal poles.

The first series of experiments had reference to the nature of the effect observed in the incandescence lamps having an insulated wire or plate placed in the vacuum.

If a platinum wire is sealed through the glass bulb of an ordinary carbon filament lamp and carries at its extremity a metal plate, so placed as to stand up between the legs of the carbon horseshoe without touching either of them, then when the lamp is actuated by a continuous current it is found that :—

(1.) This insulated metal plate is brought down instantly to the potential of the base of the negative leg of the carbon, and no sensible potential difference exists between the insulated metal plate and the negative electrode of the lamps, whether the test be made by a galvanometer, by an electrostatic voltmeter, or by a condenser.

(2.) The potential difference of the plate and the positive electrode of the lamp is exactly the same as the working potential difference of the lamp electrodes, provided this is measured electrostatically, *i.e.*, by a condenser, or by an electrostatic voltmeter taking no current,

* See 'Roy. Soc. Proc.,' vol. 38, 1885, p. 219. "On a Peculiar Behaviour of Glow Lamps when raised to High Incandescence."—In this paper Mr. Preece describes a very careful series of observations carried out with Edison incandescence lamps, and which cover the same ground as a portion of the experiments here described. The results given in (4), (7), and (11) confirm the facts which were first ascertained by him. He also arrived at the general conclusion that the phenomena so observed are due to an electric convection by matter projected from the incandescent carbon. By carrying up the working electromotive force of the lamp to a point productive of very high incandescence, he was able to measure the resulting current through a galvanometer connected between the positive lamp electrode and the middle plate corresponding to every degree of incandescence, and showed that, whilst increasing up to a certain point, the galvanometer current fell off rapidly soon after a certain critical temperature was reached, which corresponded to the appearance of a blue light or haze in the glass receiver.—[Jan. 14th, 1890.]

but if measured by a galvanometer the potential difference of the plate and the positive electrodes of the lamp is something less than that of the working lamp electrodes.

(3.) This absolute equality of potential between the negative electrode of the lamp and the insulated plate only exists when the carbon filament is in a state of vivid incandescence, and when the insulated plate is not more than an inch or so from the base of the negative leg. When the lamp is at intermediate stages of incandescence, or the plate is considerably removed from the base of the negative leg, then the plate is not brought down quite to the same potential as the negative electrode.

(4.) A galvanometer connected between the insulated plate and the *positive* electrode of the lamp shows a current increasing from zero to four or five milliampères, as the carbon is raised to its state of commercial incandescence. There is not any current greater than 0·0001 of a milliampère between the plate and *negative* electrode when the lamp has a good vacuum.

(5.) If the lamp has a bad vacuum this inequality is destroyed, and a sensitive galvanometer shows a current flowing through it when connected between the middle plate and either the positive *or* negative electrode.

(6.) When the lamp is actuated by an *alternating* current a *continuous* current is found flowing through a galvanometer, connected between the insulated plate and *either* terminal of the lamp. The direction of the current through the galvanometer is such as to show that negative electricity is flowing from the plate through the galvanometer to the lamp terminal. This is also the case in (4); but, if the lamp has a bad vacuum, then negative electricity flows *from* the plate through the galvanometer *to* the positive terminal of the lamp, and negative electricity flows *to* the plate through the galvanometer from the negative terminal of the lamp.

(7.) The same effects exist on a reduced scale when the incandescent conductor is a platinum wire instead of carbon filament. The platinum wire has to be brought up very near to its point of fusion, in order to detect the effect, but it is found that a current flows between the positive electrode of a platinum wire lamp and a platinum plate placed in the vacuum near to the negative end of that wire.

(8.) The material of which the plate is made is without influence. Platinum, aluminium, and carbon have been indifferently employed.

(9.) The active agent in producing this effect is the *negative* leg of the carbon. If the negative leg of the carbon is covered up by enclosing it in a glass tube this procedure entirely, or nearly entirely, prevents the production of a current in a galvanometer connected between the middle plate and the positive terminal of the lamp.

(10.) It is a matter of indifference whether a glass or metal tube is

employed to cover up the negative leg of the carbon; in any case this shielding destroys the effect.

(11.) If, instead of shielding the negative leg of the carbon, a mica screen is interposed between the negative leg and the side of the middle plate which faces it, then the current produced in a galvanometer connected between the positive terminal of the lamp and the middle plate is much reduced. Hardly any effect under the same circumstances is produced when the mica screen is interposed on that side of the metal plate which faces the positive leg of the carbon.

(12.) The position of the metal plate has a great influence on the magnitude of the current traversing a galvanometer connected between the metal plate and the positive terminal of the lamp. The current is greatest when the insulated metal plate is as near as possible to the base of the negative leg of the carbon, and greatest of all when it is formed into a cylinder which embraces without touching the base of the negative leg.

The current becomes very small when the insulated metal plate is removed to 4 or 5 inches from the negative leg, and becomes practically zero when the metal plate is at the end of a tube forming part of the bulb, which tube has a bend at right angles in it. Copious experiments have been made with metal plates in all kinds of positions.

(13.) The galvanometer current is greatly influenced by the surface of the metal plate, being greatly reduced when the surface of the plate is made small, or when the plate is set edgeways to the negative leg, so as to present a very small apparent surface when seen from the negative leg. In a lamp having the usual commercial vacuum, the effect is extremely small when the insulated metal plate is placed at a distance of 18 inches from the negative leg, but even then it is just sensible to a very sensitive galvanometer.

(14.) If a charged condenser has one plate connected to the insulated metal plate, and the other plate connected to any point of the circuit of the incandescent filament, this condenser is instantly discharged if the positively charged side of the condenser is connected to the insulated plate, and the negative side to the hot filament. If, however, the negative leg of the carbon horseshoe is shielded by a glass tube, this discharging power is much reduced, or altogether removed.

(15.) If the middle plate consists of a separate carbon loop, which can itself be made incandescent by a separate insulated battery, then, when this middle carbon is rendered incandescent and employed as the metal plate in the above experiment, the condenser is discharged when the negatively charged side of it is connected to the hot middle carbon, the positively charged side of it being in connexion with the principal carbon horseshoe.

(16.) If this last form of lamp is employed as in (4) the subsidiary carbon loop being used as a middle plate, and a galvanometer being connected between it and either the positive or negative main terminal of the lamp, then when the subsidiary carbon loop is cold, we get a current through the galvanometer only when it is in connexion with the positive main terminal of the lamp, but when the subsidiary carbon is made incandescent by a separate insulated battery, we get a current through the galvanometer when it is connected either to the positive *or* to the negative terminal of the lamp. In the first case the current through the galvanometer is a negative current, flowing from the middle carbon to the positive main terminal, and in the second case it is a negative current, from the negative main terminal to the middle subsidiary hot carbon.

(17.) If a lamp having a metal middle plate held between the legs of the carbon loop has a galvanometer connected between the negative main terminal of the lamp and this middle plate, we find that when the carbon is incandescent there is no sensible current flowing through the galvanometer. The vacuous space between the middle plate and the hot negative leg of the carbon possesses, however, a curious unilateral conductivity. If a single Clark cell is inserted in series with the galvanometer, we find that this cell can send a current deflecting the galvanometer when its negative pole is in connexion with the negative main terminal of the lamp, but if its positive pole is in connexion with the negative terminal of the lamp, then no current flows. The cell is thus able to force a current through the vacuous space when the direction of the cell is such as to cause negative electricity to flow across the vacuous space from the hot carbon to the cooler metal plate, but not in the reverse direction.

(18.) If a vacuum tube is constructed, having at each end horseshoe carbon filaments sealed into it, and which can each be made separately incandescent by an insulated battery, we find that such a vacuum tube, though requiring an electromotive force of many thousands of volts to force a current through it when the carbon loops are used as electrodes and are *cold*, will yet pass the current from a single Clark cell when the carbon loop which forms the negative electrode is rendered incandescent. It is thus found that a high vacuum terminated electrically by unequally heated carbon electrodes possesses an unilateral conductivity, and that electric discharge takes place freely through it under an electromotive force of a few volts when the *negative* electrode is made highly incandescent.

(19.) These experimental results above described led the writer to investigate, in the same manner, the electric arc between carbon poles taken in air. If an electric arc is formed, in the usual way, between carbon poles, and a third insulated carbon pole is allowed to dip into or touch the electric arc, or, better still, has the electric arc projected

against it by a magnet, it is found that this third or insulated pole is brought down almost to the potential of the negative carbon of the arc, and that a galvanometer connected between the third insulated carbon and the negative carbon of the arc indicates no current, but that if joined up between the positive carbon and the middle carbon a strong current of about an ampère or so is found to be passing. If an electric bell or an incandescent lamp is joined up between the third carbon and the *negative* carbon of the arc, they do not work ; but if the bell or the lamp is joined between the *positive* carbon of the arc and the third carbon, they are set in action by a strong current passing through them. These effects are produced, although the third carbon (which is best held at right angles to the other two forming the arc) is half or three quarters of an inch away from the positive and negative carbon, the sole condition being that the flame of the arc must touch or be projected by a magnet so as to touch this third carbon. We have, therefore, similar phenomena in the case of the arc and incandescence lamps.

(20.) When the electric arc is being projected against the third carbon, and has brought it down to the same potential, a galvanometer joined in between the two carbons shows no current ; but this space between the negative carbon of the arc and the third carbon possesses a unilateral conductivity, and will pass the current from a small battery of secondary cells one way, but not the other. The secondary battery when joined in series with the galvanometer sends a current, if its negative pole is in connexion with the negative carbon of the arc, and its positive pole, through the galvanometer, with the third carbon ; but if the secondary battery is reversed in position it sends no current. Negative electricity can pass along the flame-like projection of the arc *from* the hot negative carbon *to* the cooler third carbon, but not in an opposite direction.

(21.) If the arc is projected by means of a magnet for a long time against the third insulated carbon, it *craters* it out in the same fashion as the crater of the positive carbon, and the tip of this third carbon, where it has received the flame-like blast of the arc, is converted into graphite.

The same effects are observed if an iron rod is used as a third pole, and in this case the end is converted into *steel*, and rendered so hard as to be scarcely touched by the file when it has been quenched in water.

In seeking for an hypothesis to connect together these observed facts, the one which suggests itself as most in accordance with the facts is as follows :—

In the case of a carbon incandescence lamp when at vivid incandescence, carbon particles are being projected from all parts of the filament, but chiefly from the negative half of the loop. These carbon

molecules carry *negative* charges of electricity, and when they impinge upon a metal plate placed in the vacuum they can discharge themselves if this plate is positively electrified, either by being in metallic connexion with the positive electrode of the lamp or with a separate positively charged body. When the plate is simply insulated the stream of negatively charged carbon molecules brings down this insulated plate to the potential of the base of the negative leg, or to the potential of that part of the carbon conductor from which it is receiving projected molecules. These carbon molecules projected from an incandescent conductor can carry negative charges, but either cannot be positively charged, or else lose a positive charge almost instantly when projected off from the conductor.

In the case of the electric arc we must suppose that the negative carbon is projecting off a torrent of negatively electrified carbon molecules, and these, impinging against the positive carbon, wear out a crater in it by a sand-blast-like action.

The higher temperature of the positive carbon in a continuous current arc is thus explained as due to the impact of the carbon molecules projected from the negative carbon.

If the electric arc is diverted against a third insulated lateral carbon, the carbon blast from the negative carbon wears out a crater in it and brings it down to the same potential as itself. The actions going on in an electric arc may be considered to be somewhat as follows:—When the carbons are first put together, the resistance at the point of contact renders the extremities incandescent. When thus incandescent and separated, the electrification of each carbon is sufficient to begin the projection of molecules from both positive and negative carbons, probably most largely from the latter. The impact of the molecular stream from the negative pole raises the temperature of the positive carbon, and this again by radiation raises the temperature of the negative carbon end. The electromotive force is thus able to keep up a projection of negatively charged carbon molecules from the end of the negative carbon, which molecules are loosened from the mass by heat, and then move away by electric repulsion from the surface in virtue of the electric charge which they retain. It would seem as if a hot carbon molecule cannot retain a positive charge, and hence the potential difference between a third insulated carbon and the positive carbon of the arc is nearly the same as the potential difference of the positive and negative carbons of the arc. The rise of potential along the arc takes place very suddenly just in the neighbourhood of the crater of the positive carbon.

It has often been suggested that the electric arc contains a counter-electromotive force. It is questionable whether such experiments as those of Edlund ('Phil. Mag.,' vol. 36, 1868, p. 352) are entirely conclusive on this point.

It has been shown by other experimenters* that for arcs of varying length, but the same current, beyond a certain small initial length, the potential difference necessary to maintain the arc is proportional to the length of the arc plus a constant. This might thus be interpreted to mean that a certain proportion of the working electromotive force of the arc was employed in detaching the carbon molecules from the mass of the poles, and that the excess alone is represented by the current produced in an arc of definite length.

In the case of the incandescence lamps the hypothesis of the projection of negatively charged carbon molecules from the incandescent conductor, to which the name of *molecular electrovection* may be given, will suffice to explain all the various different effects produced by varying the surface, position, and distance of the metal plate against which they impinge, and also the nullifying effect of shielding this plate from the negative leg of the carbon.

That this molecular discharge goes on chiefly from the negative leg is additionally proved by the greater erosion which takes place in the deposit of carbon on the negative leg when the carbon is uniform and traversed by a continuous current.

The hypothesis that a carbon molecule detached from an incandescent carbon surface in a high vacuum can only convey away a negative charge, reconciles also the above described observed effects in which a negative discharge can be made *out* of a hot surface of carbon more easily than a positive discharge. When an electromotive force is applied to two metallic terminals or electrodes sealed into a good vacuum, it is well known that a certain initial electromotive force has to be applied before any electric current begins to flow through the gas at all. It seems conclusively proved by Mr. Crookes's researches that the nature of an electric discharge through a high vacuum consists in a torrent of electrified particles proceeding from the negative electrode. If this is the case the initial electromotive force required to begin a discharge through such rarefied gas would naturally be reduced by heating the negative electrode, so as to favour and assist the detachment of the charged molecules of that electrode. The effect of heating the negative electrode in facilitating discharge through vacuous spaces has previously been described by W. Hittorf ('Annalen der Physik und Chemie,' vol. 21, 1884, p. 90—139), and it is abundantly confirmed by the above experiments. We may say that a vacuous space bounded by two electrodes—one incandescent, and the other cold—possesses a unilateral conductivity for electric discharge when these electrodes are within a distance of the mean free path of projection of the mole-

* See Professors Ayrton and Perry, 'Proceedings of the Physical Society,' vol. 5, p. 201.

cules which the impressed electromotive force can detach and send off from the hot negative electrode.

This unilateral conductivity of vacuous spaces having unequally heated electrodes has been examined by MM. Elster and Geitel (see 'Wiedemann's Annalen,' vol. 38, 1889, p. 40), and also by Goldstein ('Wied. Ann.,' vol. 24, 1885, p. 83), who in experiments of various kinds have demonstrated that when an electric discharge across a vacuous space takes place from a carbon conductor to another electrode, the discharge takes place at lower electromotive force when the carbon conductor is the negative electrode and is rendered incandescent.

III. "A Milk Dentition in *Orycteropus*." By OLDFIELD THOMAS, Natural History Museum. Communicated by Dr. A. GÜNTHER, F.R.S. Received December 12, 1889.

[Publication deferred.]

Presents, January 9, 1890.

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January 16, 1890.

Sir G. GABRIEL STOKES, Bart., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On the Chief Line in the Spectrum of the Nebulæ." By J. NORMAN LOCKYER, F.R.S. Received December 9, 1889.

[Publication deferred.]

II. "Observations regarding the Excretion and Uses of Bile." By A. W. MAYO ROBSON, F.R.C.S. Communicated by Dr. CLIFFORD ALLBUTT, F.R.S. Received December 24, 1889.

III. "The Theory of Free Stream Lines." By J. H. MICHELL, Trin. Coll. Cam. Communicated by Professor J. J. THOMSON, F.R.S. Received January 3, 1890.

(Abstract.)

The chief object of the paper is to give a general method for the problem of free stream lines in two-dimensional motion of liquids when the boundaries are plane. The method rests on the transformation from one diagram to another by means of functions of complex variables, and, so far, is similar to that given by Kirchhoff in his 'Vorlesungen,' which is, however, of very limited application.

The first part is devoted to general theorems of transformation.

If
$$x+iy \equiv z, \qquad x-iy \equiv z',$$
$$\phi+i\psi \equiv \omega, \qquad \phi-i\psi \equiv \omega',$$

and z is a function of ω, then the transformation theorems are obtained by considering the function

$$V \equiv \log \frac{dz}{d\omega} \cdot \frac{dz'}{d\omega'}.$$

V satisfies Laplace's equation. Let the intrinsic equation of the curve ψ_0 in the z plane be

$$s = f(k),$$

where s is the arc and k the curvature.

Then V satisfies the equation

$$e^{\frac{V}{2}} = \frac{d}{d\phi} f\left(\frac{d}{d\psi} e^{-\frac{V}{2}}\right)$$

along ψ_0.

The transformation from one diagram to another is thereby reduced to solving a potential problem with a given condition over an infinite straight line.

If ψ_0 is a straight line, we have simply $dV/d\psi = 0$ along it.

By this means are given two general theorems, of which one is that of Schwarz, and the second may be stated as follows:—

The transformation

$$\frac{d\omega}{dz} = \Pi \frac{(z-z_r)^{m_r}}{(z-x_s)^{\frac{1}{2}}}$$

gives the potential of any number of infinitely long plane conductors, all in the same plane, with parallel edges and at given potentials.

Proceeding now to the hydrodynamical problem, x, y are the co-ordinates of a point of the liquid, ϕ, ψ, the velocity and stream functions.

The boundaries of the diagram in the ω plane are all straight, and therefore Schwarz's theorem will transform to a $u(\equiv p+iq)$ plane in which the boundary is $q = 0$.

Now, if v is the velocity of the fluid at (xy),

$$V \equiv \log \frac{dz}{d\omega} \cdot \frac{dz'}{d\omega'} = -\log v^2.$$

It follows that V is constant along part of the line $q = 0$ and $dV/dq = 0$ along the rest.

The transformation theorem just given enables us then to find V as a function of u, and therefore as a function of ω.

The general solution of the problem appears in the form

$$\frac{dz}{du} = \phi(u) e^{-\frac{i}{2}\int f(u)du}$$

where $\phi(u)$ and $f(u)$ are both factorial forms.

Several particular cases are next worked out, the results of which may here be given.

It will be observed that they are all of such a character that in going round the boundary of the z diagram we pass but twice from free stream line to rigid boundary.

It is only in these cases that the actual execution of the work is feasible.

Case I.—A Jet from a Vessel.

The general solution is

$$\frac{dz}{du} = \frac{1}{u-c}\Pi_n\left[\frac{1-a_n u+\sqrt{(1-a_n^2)}\sqrt{(1-u^2)}}{u-a_n}\right]^{1-\alpha_n/\pi},$$

where
$$\frac{dw}{du} = \frac{1}{u-c}.$$

Ex. I.—A rectangular vessel of given width has an aperture in the bottom.

The elimination of the unknown constants is not possible in general, but if the aperture is symmetrically placed, and d be the breadth of the vessel, c of the aperture, and k of the jet, then

$$c = k\left[1+\frac{1}{\pi}\left(\frac{d}{k}-\frac{k}{d}\right)\tan^{-1}\frac{2dk}{d^2-k^2}\right].$$

Ex. II.—Tube projecting into the bottom of a vessel. The simplest results of this case are—

(a.) When the tube is very long

$$(d-c)^2 = d(d-k),$$

with the same notation as in Ex. I.

(b.) If the tube is of small length l, and the breadth of the vessel very large

$$k = c\,\frac{\pi}{\pi+2}\left(1-\frac{4}{\pi+2}\sqrt{\frac{\pi+2}{\pi}\frac{l}{c}}\right).$$

Case II.—Flow from Pipes.

An aperture is made in one of the walls of a pipe along which water is flowing.

The formulæ of transformation are

$$\frac{dz}{du} = -\frac{au-1+\sqrt{a^2-1}\ \sqrt{u^2-1}}{(u-b)(u-c)}$$

and
$$\frac{dw}{du} = -\frac{u-a}{(u-b)(u-c)}.$$

Let d be the breadth of the pipe,
k that of the aperture,
l of the jet,
v_1 the velocity in the pipe before reaching the aperture,
v_2 the velocity after the aperture is passed,
v_3 the velocity of the jet,

then
$$d(v_1-v_2) = lv_3,$$

and
$$\frac{k}{l}\pi = \frac{1}{2}\frac{v_1+v_2}{v_3}\log\frac{(v_3+v_1)^2}{(v_3-v_1)^2}\frac{2v_3-v_1-v_2}{2v_3+v_1+v_2}$$
$$+\frac{v_3^2+v_2^2}{v_3(v_1-v_2)}\log\frac{(v_3-v_2)(v_3+v_1)}{(v_3+v_2)(v_3-v_1)}+\frac{\pi}{2}\left\{\frac{4v_3^2-(v_1+v_2)^2}{v_3^2}\right\}^{\frac{1}{2}}.$$

If the water is flowing to the aperture equally from both sides, we get
$$k = l\left[1+\frac{1}{\pi}\left(\frac{2d}{l}+\frac{l}{2d}\right)\log\frac{2d+l}{2d-l}\right].$$

If one end of the pipe is stopped, we have the case of a jet from the side of a vessel of width d, the aperture being far from the bottom.

In this case
$$\frac{k}{l}\pi = \left(\frac{d}{l}+\frac{l}{d}\right)\log\frac{d+l}{d-l}+\frac{1}{2}\frac{l}{d}\log\frac{2d-l}{2d+l}+\frac{\pi}{2}\left(4-\frac{l^2}{d^2}\right)^{\frac{1}{2}}.$$

Lastly, if the pipe is very broad, so that we have a broad stream flowing past an aperture, the result is
$$\frac{k}{l}\pi = 2\frac{v_3^2+v_1^2}{v_3^2-v_1^2}+\frac{v_1}{v_3}\log\frac{v_3+v_1}{v_3-v_1}+\pi\sqrt{\frac{v_3^2-v_1^2}{v_3^2}},$$

v_3 being the velocity of the jet, and v_1 the velocity of the stream.

Case III.—Impact of a Stream against a Plane.

The stream impinges at a given angle against an infinite plane. If x be measured along the plane, the equations of the boundaries of the stream are
$$\left.\begin{aligned} x &= (1+a)\log\cos\tfrac{1}{2}\theta-(1-a)\log\sin\tfrac{1}{2}\theta-a\log\cos\theta \\ y &= \sqrt{(1-a^2)}\log\cot\tfrac{1}{2}(\tfrac{1}{2}\pi-\theta)+c \end{aligned}\right\}$$

and
$$\left.\begin{aligned} x &= (1+a)\log\sin\tfrac{1}{2}\theta-(1-a)\log\cos\tfrac{1}{2}\theta-a\log\cos\theta \\ y &= \sqrt{(1-a^2)}\log\cot\tfrac{1}{2}(\tfrac{1}{2}\pi-\theta)+c' \end{aligned}\right\},$$

where θ in both lies between 0 and $\frac{1}{2}\pi$, and $\cos^{-1} a$ is the inclination of the stream to the plane.

In the second part of the paper, some general transformation theorems are obtained, which are applicable to problems of electric condensers, forms of hollow vortices, &c.

If two polygons lie one within the other, the transformation of the area between them which makes the boundaries ψ curve is

$$\frac{dz}{d\omega} = \Pi_r\{\Theta[a(\omega-\omega_r)]\,\mathrm{H}[a(\omega-\omega_r)]\}^{\frac{\alpha_r}{\pi}-1},$$

where α_r is the internal angle of the polygon at $\omega = \omega_r$, and Θ, H are the elliptic functions usually so indicated.

A similar transformation is given for the case in which one polygon lies outside the other. The method is then applied to find the form of hollow vortices in certain cases. The transformation which gives the motion due to a stationary hollow vortex between two parallel planes is

$$z = \mathrm{A} \log \mathrm{tn}\,(\omega - \tfrac{1}{2}i\mathrm{K}').$$

Presents, January 16, 1890.

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January 23, 1890.

Sir G. GABRIEL STOKES, Bart., President, in the Chair.

The President announced that at the next meeting a Member of Council would be balloted for in place of the Rev. S. J. Perry, deceased.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On a Photographic Method for determining Variability in Stars." By ISAAC ROBERTS, F.R.A.S. Communicated by Professor J. NORMAN LOCKYER, F.R.S. Received January 14, 1890.

(Abstract.)

Some of the uncertainties which necessarily attend the determination of variability in the brightness of stars by eye observations are removed by the application of photographic methods, and particularly by that of giving two or more exposures of the same photographic plate to a given sky space, with intervals of days or weeks between each exposure.

In this way any errors caused by atmospheric, actinic, or chemical changes, together with those due to personal bias, are eliminated, and the study of stellar variability can be pursued under conditions that admit of the necessary exactitude.

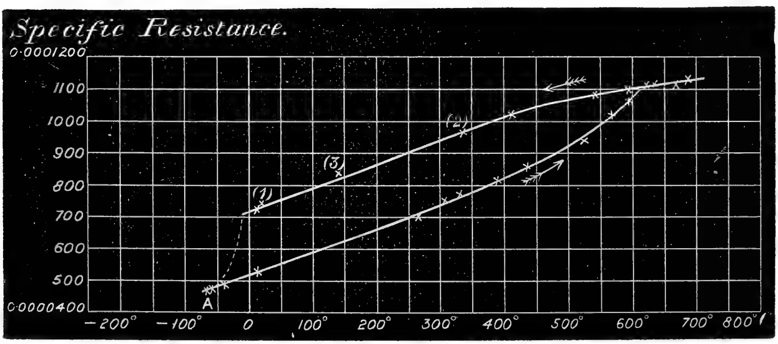
As an illustration of the applicability of this dual photographic method, the enlargement on paper from the negative, which accompanies the full paper, shows the results obtained by two exposures of the same plate to the sky in the region of the great nebula in Orion. The first exposure was of two hours' duration on the 29th January, and the second of two and a half hours on the 3rd February, 1889. The stellar images formed during the two exposures are 0·0122 of an inch apart, measured from centre to centre, and are therefore comparable with each other in the field of a microscope. When the images are examined in the manner thus indicated and their diameters also measured by means of a suitably made eye-piece micrometer, it is found that at least ten of the photographed stars, the magnitudes of which are estimated to range between the 7th and 15th, have changed to a considerable extent in the short interval of five days.

The ten stars referred to are to be found within an area of less than two square degrees of the sky, and in the table given are the co-ordinates of their positions with reference to *theta Orionis*. The measurements of the diameters of their photo-images on a scale of 0·00002 of an inch are also given.

II. "Physical Properties of Nickel Steel." By J. Hopkinson, D.Sc., F.R.S. Received January 16, 1890.

Mr. Riley, of the Steel Company of Scotland, has kindly sent me samples of wire drawn from the material concerning the magnetic properties of which I recently made a communication to the Royal Society. As already stated, this material contains 25 per cent. of nickel and about 74 per cent. of iron, and over a range of temperature from something below freezing to 580° C. it can exist in two states, magnetic and non-magnetic.

The wire as sent to me was magnetisable as tested by means of a magnet in the ordinary way. On heating it to a dull redness it became non-magnetisable whether it was cooled slowly or exceedingly rapidly by plunging it into water. A quantity of the wire was brought into the non-magnetisable state by heating it, and allowing it to cool. The electric resistance of a portion of this wire, about 5 metres in length, was ascertained in terms of the temperature; it was first of all tried at the ordinary temperature, and at temperatures up to 340° C. The specific resistances at these temperatures are indicated in the curve by the numbers 1, 2, 3. The wire was then



cooled by means of solid carbonic acid, the supposed course of change of resistance is indicated by the dotted line on the curve, the actual observations of resistance, however, are indicated by the crosses in

the neighbourhood of the letter A on the curve. The wire was then allowed to return to the temperature of the room, and was subsequently heated, the actual observations being shown by crosses on the lower branch of the curve; the heating was continued to a temperature of 680° C., and the metal was then allowed to cool, the actual observations being still shown by crosses. From this curve, it will be seen that in the two states of the metal, magnetisable and non-magnetisable, the resistances at ordinary temperatures are quite different. The specific resistance in the magnetisable condition is about 0·000052, in the non-magnetisable condition it is about 0·000072. The curve of resistance in terms of the temperature of the material in the magnetisable condition has a close resemblance to that of soft iron, excepting that the coefficient of variation is much smaller, as, indeed, one would expect it to be in the case of an alloy; at 20° C. the coefficient is about 0·00132, just below 600° C. it is about 0·0040, and above 600° it has fallen to a value less than that which it had at 20° C. The change in electrical resistance effected by cooling is almost as remarkable as the change in the magnetic properties.

Samples of the wire were next tested in Professor Kennedy's laboratory for mechanical strength. Five samples of the wire were taken which had been heated and were in the non-magnetisable state, and five which had been cooled and were in the magnetisable state. There was a marked difference in the hardness of these two samples; the non-magnetisable was extremely soft, and the magnetisable tolerably hard. Of the five non-magnetisable samples the highest breaking stress was 50·52 tons per square inch, the lowest 48·75; the greatest extension was 33·3 per cent., the lowest 30 per cent. Of the magnetisable samples, the highest breaking stress was 88·12 tons per square inch, the lowest was 85·76; the highest extension was 8·33, the lowest 6·70. The broken fragments, both of the wire which had originally been magnetisable and that which had been non-magnetisable, were now found to be magnetisable. If this material could be produced at a lower cost, these facts would have a very important bearing. As a mild steel the non-magnetisable material is very fine, having so high a breaking stress for so great an elongation at rupture. Suppose it were used for any purpose for which a mild steel is suitable on account of this considerable elongation at rupture, if exposed to a sharp frost its properties would be completely changed—it would become essentially a hard steel, and it would remain a hard steel until it had actually been heated to a temperature of about 600° C.

Presents, January 23, 1890.

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January 30, 1890.

Sir G. GABRIEL STOKES, Bart., President, in the Chair.

In pursuance of notice sent to the Fellows, an election was held to fill the vacancy upon the Council occasioned by the death of the Rev. S. J. Perry.

The Statutes relating to the election of the Council and the Statute relating to the election of a Member of Council upon the occurrence of a vacancy were read, and Mr. Hulke and Mr. Stainton having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were taken, and Mr. William Henry Mahoney Christie, Astronomer Royal, was declared duly elected.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Investigations into the Effects of Training Walls in an Estuary like the Mersey." By L. F. VERNON-HARCOURT, M.A., M.Inst.C.E. Communicated by A. G. VERNON-HARCOURT, F.R.S. Received January 21, 1890.

(Abstract.)

A description was given in a previous paper of the results of experiments with training walls in a working model of the tidal Seine;* and the present investigations were carried out with a similar working

* 'Proceedings of the Royal Society,' vol. 45, p. 504, and Plates 2 to 4.

model of the Mersey estuary, from near Warrington to the open sea beyond the bar, made to a horizontal scale of $\frac{1}{30000}$, and a vertical scale of $\frac{1}{500}$, and with a bed formed of fine Bagshot sand. The experiments were directed to the solution of two problems, namely, (1) The influence of training walls in the wide upper estuary on the channel below Liverpool, and across the bar; and (2) The effects of training works in the lower estuary on the channel across the bar.

The model was first worked, without modification, till a fair reproduction was obtained of the existing conditions of the estuary. Training walls, made of strips of tin, were then inserted in the model, following the lines of the Manchester Ship-Canal Scheme of 1884, down the middle of the upper estuary, for which the present line of canal, in course of construction, skirting the Cheshire shore, was substituted in 1885. This modification soon produced a change in the model, which no previous working had effected; for, though the channel between the training walls was deepened, the upper estuary began to silt up as the working of the model proceeded, and the channel immediately below Liverpool began to shoal, till at last the main navigation channel below the "narrows" became very shallow for some distance. This result solves a very much disputed question as to the effects of these training works, and, together with the results of the Seine model, affords grounds for the conclusion that training walls placed in a sandy estuary, where sandbanks exist below them, will lead to accretion behind them. As the channel in the lower estuary of the Mersey is almost wholly maintained by the tidal water flowing into and out of the estuary above, accretion in the upper estuary would necessarily produce a deterioration in the channel below.

The model was next restored to its original form, and training walls were inserted in the lower estuary, in continuation of the narrow channel between Liverpool and Birkenhead, gradually diverging out with a trumpet-shaped outlet, so as not to impede the tidal influx into the upper estuary. The scour through this trained channel gradually washed away a sandbank to the north of the outlet, and eventually formed a channel to the north of the previous channel, towards Formby Point, in the model, with a minimum depth considerably more than was previously obtained across the bar.

The bed of the model was then restored to its previous state; but the training walls in the lower estuary were retained, and a sandbank impeding the outlet was removed to the level of the bar. The scour through the trained channel, with this arrangement, produced a more direct and uniform channel than in the previous case, with a similar increase in depth.

These experiments indicate that, whereas training walls in the upper estuary would be injurious, owing to the resulting accretion, training walls in the lower estuary would improve the depth of the outlet channel; and that such training walls, combined with dredging, offer the best prospect of forming a direct stable, and deepened channel across the bar.

II. "On outlying Nerve-cells in the Mammalian Spinal Cord." By Ch. S. Sherrington, M.A., M.B., &c. Communicated by Professor M. Foster, Sec. R.S. Received January 30, 1890.

(Abstract.)

Gaskell has shown* that in the cord of the alligator scattered nerve-cells are to be seen at the periphery of the lateral column. Although nerve-cells appear to be absent from that position in the spinal cord of Mammalia as represented by the rabbit, cat, dog, calf, monkey, and man, yet there are in these animals isolated nerve-cells present in the white matter of the cord, not only in the deeper portions of the lateral column, but in the anterior and posterior columns as well.

In the anterior columns occasional nerve-cells, of the multipolar kind, lie among those fibre-bundles which pass between the deeper mesial border of the anterior horn and the anterior commissure at the base of the anterior fissure. They, in the instances observed, are smaller than the large cells characteristic of the anterior horn, and lie with two of the processes directed parallel with the horizontal transverse fibres among which they are placed. Such cells have been observed in the human cord and in the cord of the dog and bonnet monkey.

In the lateral column, of the spinal cord of man and the other animals named above, it is common to find outlying members of the group of small cells of the lateral horn, Clarke's tractus intermedio-lateralis, situated in the white matter, distinctly beyond the limits of the grey. Some outlying cells here are placed at a great distance from the grey. These are all probably to be considered members of the intermedio-lateral group. Their similarity to those cells in form and size is striking. They are generally placed upon, or at least in close connexion with, the fine connective-tissue septa which pass across the white matter. It is probable that the cells are connected with the medullated nerve-fibres running along these septa. The cells are fusiform, with the longer axis parallel to the direction of the nerve-fibres running in the septa.

In the part of the lateral column adjacent to the lateral reticular

* 'Proceedings of the Physiological Society,' 1885.

formation numerous nerve-cells are to be found among the interlacing bands of nerve-fibres. These are often fusiform, but in many cases multipolar; they are for the most part small, but occasional large individuals can be found; the latter would appear always to be multipolar. Where the lateral column comes into contact with the lateral limb of the substantia gelatinosa of the caput cornu posterioris ganglion-cells can frequently be seen in it. The larger axis of these cells is parallel to the outline of the caput cornu. They seem to exist most numerously in regions, such as the lumbo-sacral, in which medullated fibres, probably posterior root-fibres, sweep through the deeper part of the lateral column round the lateral limb of the gelatinosa as if to reach the base of the posterior horn.

In the posterior columns outlying nerve-cells are also to be found, especially in the human cord. In these columns the cells appear to be outstanding members of the posterior vesicular group of Clarke. They are best seen in the upper lumbar and lower dorsal regions. They are large, measuring in some instances 70 μ across. In appearance they closely resemble the cells of Clarke's column. They are nearly always of broadly ovate shape. They appear always to lie on or in close relation to those horizontal bundles of nerve-fibres which curve in a ventro-lateral direction from the depth of the extero-posterior column into the grey matter in the neighbourhood of the posterior vesicular group. The longer axis of the cell is placed parallel to the nerve-fibres it lies upon or among. Where a process from the bipolar cell-body can be followed, it disappears in a direction which is that of the surrounding nerve-fibres. The cell would seem in the majority of cases to lie with its length in a plane at right angles to the long axis of the cord. Frequently the cells lie close to the grey substance of Clarke's column, but in some specimens they occupy positions far removed from the grey matter; they may even lie near the periphery of the extero-posterior column.

The chief interest attaching to nerve-cells lying in the white matter of the spinal cord is that they may be supposed to be connected with the nerve-fibres among which they are, and that from that fact some knowledge may be gained as to the anatomy of themselves, and of the group of which they may be outlying individuals, or of the fibre-bundles containing them.

With regard to the cells existing among fibres passing to the white commissure of the cord, it is legitimate to consider their presence as evidence in favour of the view that some of the cells of the median portion of the ventral grey horn are directly connected with medullated fibres passing to or from the opposite half of the cord by way of the anterior commissure.

The cells in the lateral column outside the lateral horn may be taken to point to the connection of the intermedio-lateral group of

Clarke with the nerve-fibres which radiate in bundles from the grey matter of that region into the lateral column, and to show that some of the fibres with which these are related pass out transversely well into that area which is occupied almost exclusively (man) by fibres of the crossed pyramidal tract. Concerning some of the outlying cells in the more dorsal portion of the lateral column, the same inferences may be drawn; and some of them would seem to be connected with fibres of the posterior roots that curve round the lateral aspect of the caput cornu posterioris. Of the outlying cells in the posterior column, if they are outlying members of Clarke's group, the relations which they suggest for that group are—

i. That the group is connected *directly* with certain of the median fibres of the posterior spinal roots, namely, those which after an upward course in Burdach's column plunge into the grey matter of the base of the posterior horn.

ii. That some at least of the cells of that group are interpolated, more or less immediately, into the course of medullated nerve-fibres of large calibre.

The question naturally arises, May not these cells in the posterior column of the Mammalian cord represent the bipolar cells discovered by Freud,* in the cord of *Petromyzon Planeri*, to be in direct communication with fibres of the posterior roots? If so may Clarke's column be considered a portion of the ganglion of the posterior spinal nerve-root which has been retained in the interior of the spinal cord in the thoracic and certain other regions?

III. "On the Germination of the Seed of the Castor-oil Plant (*Ricinus communis*)." By J. R. Green, M.A., B.Sc., F.L.S., Professor of Botany to the Pharmaceutical Society of Great Britain. Communicated by Professor M. Foster, Sec. R.S. Received January 29, 1890.

(Abstract.)

The older views of the transformations of the reserve products of this plant, as advanced by Sachs and other writers, took account only of the oil present in the cells, and were briefly, that it undergoes by oxidation a conversion into carbohydrate, the idea of this change being chiefly based on the observation that as the oil disappears from the endosperm during germination, starch appears in various parts of the embryo. Later writers have suggested the existence of a ferment, splitting up the fat into glycerine and fatty acid, and the further transformation of the latter into the starch.

* Freud, 'Vienna Sitzungsberichte,' January, 1877.

The work embodied in this paper deals (*a*) with the agencies which, during germination, render the reserve materials available for the use of the embryo, (*b*) with the forms in which these are absorbed by it and the mode of their absorption, and (*c*) with the parts played in the process by the endosperm and the embryo respectively.

1. *The agencies at work.*—A ferment is found to exist as a zymogen in the resting seed, which is readily developed by warmth and weak acids into an active condition. The results of its activity are the splitting up of the fat with formation of glycerine and (chiefly) ricinoleic acid. Further changes, brought about by the protoplasm of the endosperm cells, form from the latter a lower carbon acid which, unlike ricinoleic acid, is soluble in water and is crystalline. These changes do not take place in the absence of free oxygen. A quantity of sugar also is formed, which appears to have the glycerine as its antecedent.

The proteids of the seed, which consist of globulin and albumose, are split up by another ferment, with formation of peptone and asparagin. This ferment resembles closely the ferment previously described by the writer as occurring in germinating lupin seeds.

2. *The forms in which the reserve materials are absorbed.*—Examination of the seeds during the whole course of absorption shows that the only products which enter the embryo are a crystalline acid, sugar, possibly some peptone, and asparagin. Consideration of the structure of the cotyledons, which are the absorbing organs, shows that the mode of absorption is always dialysis, a view antagonistic to that of Sachs, who has put forward the idea of a penetration of the cell walls by the unchanged oil. It follows from this that the starch seen by him and other observers in the tissues of the young embryo was the result of a re-formation from the diffusible bodies now traced.

3. *The relative influence of the endosperm and the embryo.*—The changes are found to be initiated in the endosperm, for they take place, though more slowly, when the embryo is carefully removed. The latter has, however, an influence upon the process, germination being more rapid when it, or even part of it, is left in contact with the endosperm. This is shown not to be due to simple removal of the products of the decompositions, but is rather to be regarded as due to a stimulus of a physiological nature caused by the commencing development of the embryo.

4. An additional point of interest in the progress of the germination is the liberation in the endosperm of a rennet ferment of considerable vigour. At present an explanation of the action of this is difficult, though experiments are still proceeding with a view to clearing it up.

Presents, January 30, 1890.

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"The Assimilation of Carbon by Green Plants from certain Organic Compounds." By E. HAMILTON ACTON, M.A., Fellow of St. John's College, Cambridge. Communicated by W. T. THISELTON DYER, C.M.G., F.R.S. Received April 20,—Read May 16, 1889.

The recent synthesis of a true glucose ("acrose") by Fischer and Tafel,* from acroleïn (acrylic aldehyde) and also from glycerin,† in conjunction with the additions to our knowledge of the constitution of dextrose and lævulose by Kiliani,‡ &c., suggests fresh attention to the "aldehyde theory" regarding the synthetical formation of carbohydrate in green plants.

It is now widely believed by vegetable physiologists that a glucose is produced in the first instance from CO_2 and H_2O, but the nature of the intermediate changes is still uncertain. The experiments described in this paper were commenced to ascertain whether starch can be produced in the assimilating cells of a green plant by supplying it with acroleïn or closely related bodies, and subsequently extended to other organic compounds related to carbohydrates.

The well-known theory§ that formic aldehyde (HCOH) is first produced from CO_2 and H_2O, and then becomes polymerised to glucose, has not yet received any direct experimental proof, although it is stated by Reinke‖ that formic aldehyde has been detected in the product obtained by distilling the leaves of several plants with water.

The artificial polymerisation of formic aldehyde appears to yield a complex mixture of aldehyde and ketone alcohols, which has been variously described as methylenitan, formose, pseudo-formose, &c. Quite recently Fischer¶ and Loew** have independently stated that a small quantity of a true glucose can be proved to occur in "formose" by the phenylhydrazine reaction.†† According to Loew this polymerisation only occurs with dilute solutions of the aldehyde, and better with PbO or $Pb(OH)_2$ than $Ca(OH)_2$.

Loew supports the view that formic aldehyde is formed as an

* 'Berichte der Deutsch. Chem. Gesell.,' 1888, pp. 1088, 2566.

† *Ibid.*, p. 3384.

‡ *Ibid.*, p. 221.

§ Compare Vines, 'Physiology of Plants,' Lecture 9, Cambridge, 1886.

‖ 'Berichte der Deutsch. Chem. Gesell.,' 1881, p. 2144.

¶ *Ibid.*, 1889, p. 359.

** *Ibid.*, 1889, p. 470.

†† For an account of the views as to nature of formose, pseudoformose, methylenitan, &c., generally held before the publication of Fischer and Loew's papers referred to above, see Tollens, 'Handbuch der Kohlenhydrate;' Sec. IV, 250—252, &c. Breslau, 1888.

intermediate product in the synthesis of carbohydrate by green plants from CO_2 and H_2O, but that it becomes immediately polymerised at the moment of formation; he does not, however, adduce any new physiological experiments.

Wehmer* has shown that assimilating plant cells do not form starch from solutions of formic aldehyde or formose, and A. Meyer that this is also true for solutions of aldehyde (acetic) and trioxymethylene.

A. Meyer's paper ('Botan. Zeitung,' 1886, pp. 81, 105, 129, 145) is frequently referred to throughout the following pages.

It is well known† that starch is formed by the leaves of green plants when they are supplied with solutions of glucose or cane-sugar (saccharon): but very few experiments have been made to ascertain how far this is true for other organic compounds. A. Meyer has extended such investigations to the behaviour of leaves placed in solutions of other carbohydrates and a few other compounds; his researches are frequently referred to throughout the following pages. He found that starch is formed by leaves placed in solutions of glucose, saccharon, mannite, inulin, and glycerin.

Meyer's method consisted in placing leaves which had been deprived of starch in the dark in solutions of the substances. I have extended the investigation to other substances—especially aldehydes and substances related chemically to carbohydrates, using different methods and devoting especial attention to the formation or not of starch in the leaves of green plants when organic substances are supplied through the medium of their roots and not directly to the leaves.

E. Laurent‡ has confirmed Meyer's observation that starch is formed from glycerin.

Wehmer's negative results with formic aldehyde and formose have been already alluded to.

Meyer (*loc. cit.*) has shown that starch is not formed by leaves from solutions of raffinose, inosite, erythrite, dulcite,§ trioxymethylene, aldehyde (acetic).

Neither Meyer, Laurent, nor Wehmer describes any experiments with reference to the supply of the substances used to the roots of plants.

In the following pages where the compounds used have also been employed by Meyer, as described above, his results are stated in giving details of experiments, but I did not generally make observa-

* 'Berichte der Deutsch. Chem. Gesell.,' 1887, p. 2614.

† Meyer's paper gives full references to previous experiments on this point.

‡ 'Botan. Zeitung,' 1886, p. 751.

§ Full information concerning the relation of these bodies to the glucoses is given by Tollens ('Handbuch der Kohlenhydrate').

tions on shoots (*vide* groups A and C in detailed account) where Meyer has obtained positive results.

I am indebted to Professor S. H. Vines for the suggestion to experiment with an "extract of natural humus" (see No. 12, p. 172).

Methods and Apparatus Employed.

The experiments with each of the substances employed are classified as follows:—

A. Experiments with cut branches.

B. Experiments with solutions supplied to the roots of plants placed in a culture liquid, or in a few cases in sand moistened with the same.

C. Experiments where the solutions were applied externally by placing on the upper surfaces of leaves.

All the investigations were made as far as possible with plants, shoots, leaves, &c., in a healthy condition, and results are not stated in any cases where there was reason to believe that the plants, &c., had been injured by preliminary manipulations or exceptionally unfavourable conditions during the progress of the trials.

As is generally the case in experiments with culture solutions, algæ, fungi, &c., often developed in the solutions, although the cylinders were surrounded with black paper and the liquids had been previously boiled; where this occurred to any considerable extent the cultures were repeated with fresh plants. In a few cases the roots of the plants were placed in damp sand and the sand moistened with the substances in use. The sand had been strongly heated in a muffle just before using. The words "sand culture," placed against some of the results in the detailed account, signify that this method had been used instead of the ordinary water culture.

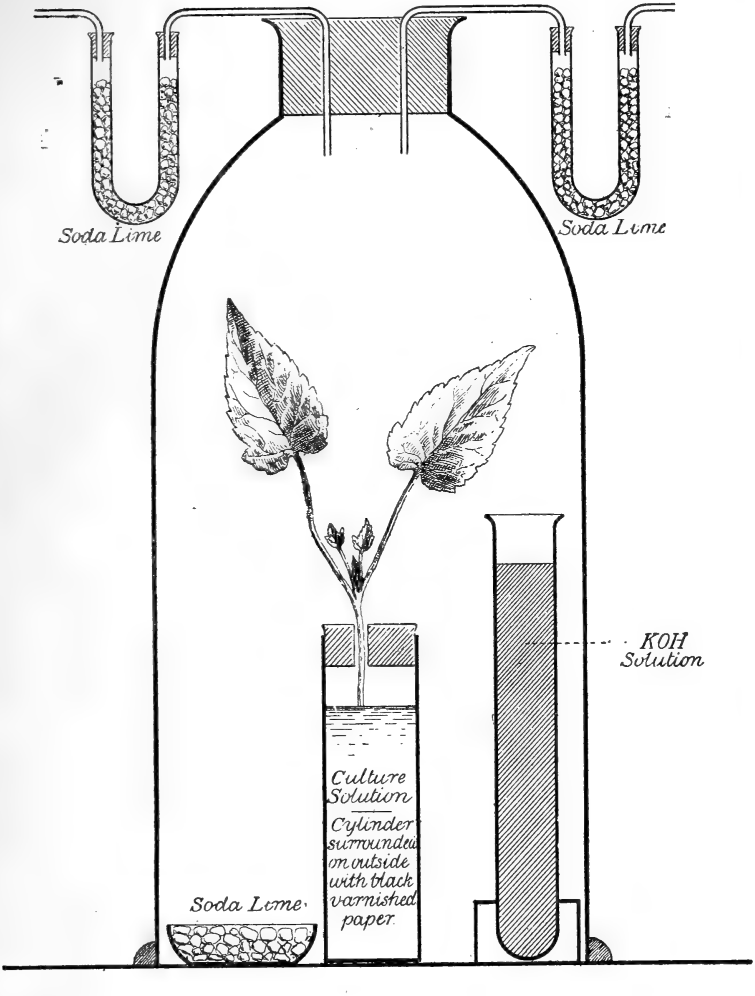
To deprive the leaves and tissues of starch at the beginning of the experiments, two methods were resorted to:—

(1.) Placing in the dark until portions of the leaves were shown by testing to be completely free from starch.

(2.) Placing under a bell-jar with substances which entirely remove all CO_2 from the air until the same result was obtained. Of these methods the latter was found to be the more convenient and used in nearly all cases. In the case of seedling plants the cotyledons must obviously be removed before placing under the bell-jar; but this operation need not cause any injury to the plant if carefully performed and the young plant has formed sufficient ordinary foliage leaves to be independent of the cotyledons. The apparatus used was as described on p. 154, and is represented in section by diagram No. 1 on the opposite page.

In this apparatus the branches, shoots, &c., freshly cut off under

DIAGRAM No. 1.



water were placed in a cylinder containing the culture solution and the seedling plants either in the same or in a cylinder containing damp sand moistened with the culture solution. When it had been found by testing portions of the tissues that the plants, &c., were entirely free from starch, they were at once transferred to fresh cylinders containing the different solutions and placed under other bell-jars similarly fitted.

The apparatus shown in diagram No. 1 and described on the next page was always used in the first instance, but where positive results

were obtained the trials were repeated, using the modified cylinder described below and figured on p. 155 (diagram No. 2).

In testing the tissues for starch Sachs' well-known method was used; in cases where the results were negative the "potash method" recommended for small quantities of the substance was employed. I generally also made a micro-chemical examination of portions of the tissues in addition to the direct tests.

The bell-jar (see diagram No. 1) is accurately ground to fit the glass plate, the surfaces in contact are covered with a mixture of vaseline, resin, and beeswax, which is extremely tenacious, and entirely prevents any access of air in this direction.*

The india-rubber stopper is perforated with two holes, through which glass tubes are inserted connected with soda-lime U-tubes; this arrangement allows a free circulation between air in the bell-jar and external atmosphere, but entirely deprives any air entering the apparatus of CO_2. Any CO_2 derived from respiration of the plants is at once absorbed by the KOH or soda-lime, so that the air in the bell-jar is entirely destitute of CO_2 during the whole course of experiment.

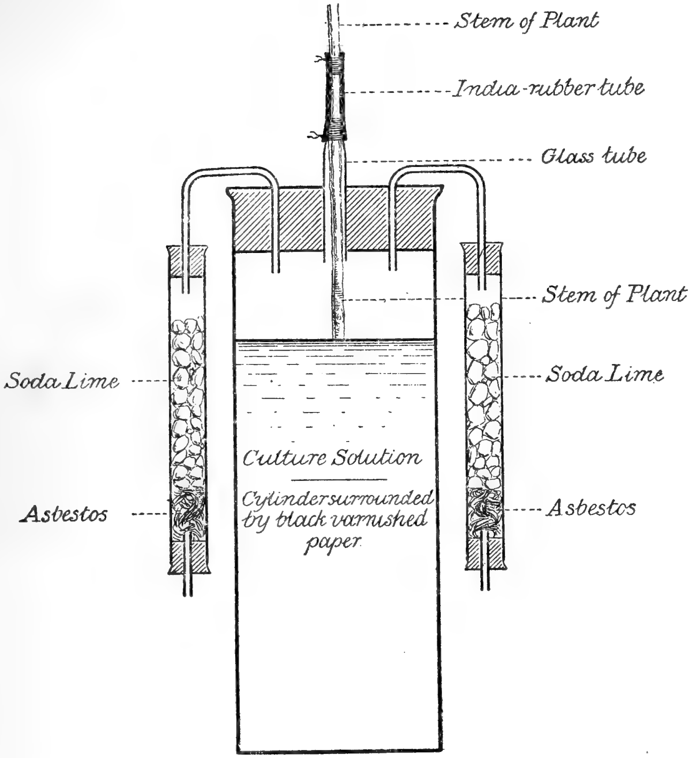
Two vessels of water (not shown in diagram No. 1) are also placed under the bell-jar to prevent any chance of the air being rendered too dry by the soda-lime. The water in these vessels is deprived of any CO_2 which it may contain in solution by the addition of baryta-water ($Ba(OH)_2$).

Since the experiments showing a positive result are open to the possible objection that CO_2 might be evolved by decomposition in the solution, and be absorbed by the leaves before it was taken up by the soda-lime or potash, I repeated these with the modified apparatus shown on the opposite page (diagram No. 2).

In this case any CO_2 evolved from decompositions in the culture solution could not find its way to the leaves; but, at the same time, a free circulation of air is allowed between the space at top of the cylinder and that in the bell-jar. Except for this modification in the cylinder containing the culture solution, the apparatus is the same as described above; instead of the plant stem being simply passed through a hole in cork of cylinder, the insertion is made gas-tight, as

* Sachs, Godlewski, &c., in similar experiments close the bottom of the bell-jar by placing it in a dish containing strong KOH solution, through which they introduce tubes (curved) to allow a free circulation of air, &c. The arrangement described in the text is equally efficacious and more convenient to employ, as the cylinder, dishes, &c., stand on a glass plate instead of in KOH solution. In the first experiment with each apparatus used, a portion of the air was withdrawn from the bell-jar, collected over mercury, and tested for CO_2 by the ordinary methods of gas analysis at various intervals; in all cases the air in the apparatus was found to be completely free from CO_2.

DIAGRAM NO. 2.



shown in diagram, by a glass tube—fitting into the cork—having a piece of india-rubber tubing slipped over its end and a portion of the stem, fastened with fine copper binding wire in each case. Communication between the air in cylinder and that of bell-jar is provided by means of the side tubes, although these prevent the exit of any CO_2 from the cylinder.

Method for Experiments with Anacharis alsinastrum *and Water Plants.**

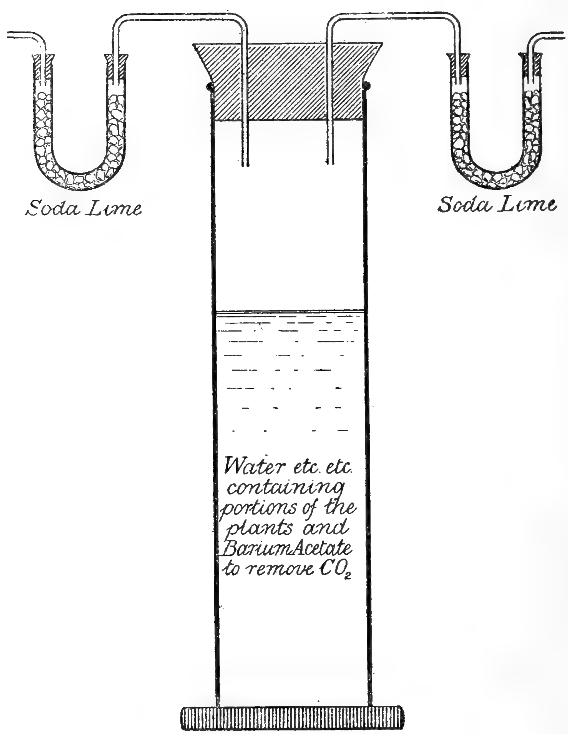
Pieces of the plant, about 8—15 cms. long, were placed in distilled water which had been deprived of any dissolved CO_2 by the addition

* In experiments with cut branches of plants I consider that the conditions are more nearly normal with water than land plants, but owing to the difficulty of keeping a supply of water plants under the requisite conditions, I have not made any very extensive use of them in these experiments.

of barium acetate solution, sufficient excess of the latter being present to withdraw from the water any CO_2 obtained from respiration. The jars containing water and plants were exposed in a window—receiving some direct sunlight—for two days, at the end of which period leaves tested as described were found to contain no traces of starch.

The plants were then rapidly transferred to other jars containing the same solutions as used in the previous experiments (see next page), with the addition of sufficient barium acetate to leave an excess of the salt for withdrawal of any CO_2 formed during the experiment, but in no case did the amount of soluble barium salts at the beginning of the experiment exceed 2·5 per cent. barium (4·2 per cent. $BaSO_4$ on precipitation). The jars in this and the previous experiment were closed by tightly fitting india-rubber corks perforated with two holes through which are inserted glass tubes connected with soda-lime U-tubes, to allow a free circulation of air in the space above the water, but to deprive any air so entering the apparatus of all traces of CO_2 (diagram No. 3).

DIAGRAM No. 3.



The plants used for these experiments were—

Shoots (cut branches) of—

Acer pseudoplatanus, L.; *Phaseolus vulgaris*, L.; *Ranunculus acris*, L.; *Cheiranthus Cheiri*, L.; *Tilia Europœa*, L.; *Alisma plantago*, L.; *Scrophularia aquatica*, L.

Seedling plants (entire) of—

Acer pseudoplatanus, L.; *Phaseolus vulgaris*, L.; *Ph. multiflorus*, L.; *Cheiranthus Cheiri*, L.; *Quercus robur*, L.; *Campanula glomerata*, L.; *Euphorbia helioscopia*, L.; *Epilobium hirsutum*, L.

Water plants.—Shoots of—

Anacharis alsinastrum, Bab.; *Callitriche aquatica*, Sm.; *Fontinalis antipyretica*, L.; *Chara vulgaris*, L.; *Sparganium natans*, Bab.

The plants made use of in these experiments were not selected for any particular reason beyond the fact that I had an abundant supply of them at hand in the ground behind St. John's College Laboratory, where all these experiments were conducted.

The seedling plants of *Tilia*, *Acer*, *Cheiranthus*, *Campanula*, *Euphorbia* were all obtained from the place mentioned. Those of *Quercus* were brought from a neighbouring field, and planted in the garden in front of the laboratory.

The young plants of *Phaseolus multiflorus* and *P. vulgaris* were raised from seed in damp sawdust, and planted out till required for use.

The other plants were growing in the garden and immediate vicinity. As I did not in any cases find the results differing with the plants used where I considered the experiments had been satisfactorily carried out, I selected those which seemed best adapted to the apparatus in each case.

The solution used for the culture of the plants, and referred to throughout as the "culture solution," was prepared so as to contain the weights given below in 100 c.c. of the liquid.

Potassium nitrate (KNO_3)........	0·15	grams.
Magnesium chloride ($MgCl_2$)......	0·10	,,
Calcium phosphate ($Ca_3(PO_4)_2$)....	0·05	,,
Ferrous sulphate ($FeSO_4$)	0·025	,,
Calcium sulphate ($CaSO_4$)........	0·05	,,
Water (distilled)................	100	,,

In the case of the water plants there was added to the above 3—4 per cent. of barium acetate (to remove CO_2), as mentioned in describing the apparatus for water plants, which would cause some alterations in the soluble salts. Such a solution contains all the elements necessary for normal growth of a plant except carbon.

No. 1.—*Experiments with Acroleïn.*

The acroleïn was prepared by the usual method,* viz., distillation of glycerin and acid potassium sulphate, the distillate being in the first instance collected in a receiver over PbO and $CaCl_2$ to remove acrylic acid and water. The product was three times "rectified" over $CaCl_2$, and preserved in a sealed tube over a few fragments of $CaCl_2$ till required for use.

The "acroleïn-ammonia" was prepared by Claus's method,† viz., acroleïn vapour was passed into strong aqueous ammonia, the excess of ammonia driven off by warming, and the reddish solid acroleïn-ammonia precipitated by addition of excess of ether alcohol.

The solid was dissolved in water immediately before use.

Acroleïn-ammonia is a condensation product having the formula C_6H_9NO. ($2C_3H_4O + NH_3 = C_6H_9NO + H_2O$.)

The crystals of acid sodium sulphite compound, of which the crystals are somewhat insoluble, can be easily prepared in the ordinary way for these compounds. Composition, $2NaHSO_3 \cdot C_3H_4O$.

Owing to the fact that acroleïn is very liable to undergo spontaneous decomposition on standing in contact with water, and the extremely offensive nature of the substance in a free state, I employed certain soluble acroleïn compounds, as well as the uncombined aldehyde; but, although these compounds did not act so prejudicially on the plants, they caused them to assume an unhealthy appearance after 4—5 days, and two out of six plants which had been grown in the solutions under the conditions described on p. 160 failed to recover when again planted under normal circumstances, although fully supplied with water and shaded from intense direct sunlight.

Details of these experiments are given on the following pages. I think they prove conclusively that plants are unable to form starch in their leaves from acroleïn or its compounds when supplied to them as such, and that it is therefore doubtful whether the synthesis of glucose by Fischer and Tafel from acroleïn has any direct bearing on the formation of starch in plants. The results with ordinary aldehyde (acetaldehyde) and some of its compounds were also negative (see pp. 163—164), although these did not seem to have any injurious effect on the plants. In 1 per cent. solution no formation of starch could be detected, whether the substance was supplied to the roots, cut branches, or the external surface of the leaves.

No. 1.—*Experiments with Acroleïn.*

I. With free aldehyde—

* See F. Beilstein, 'Handbuch der organischen Chemie,' 2nd Edit., p. 360.

† 'Liebig's Annalen,' vol. 130, 1864, p. 185.

A. On Cut Branches.

Solution used.	Plants.	Results.
The culture solution + 0·2 per cent. acroleïn	*Acer pseudoplatanus* *Phaseolus vulgaris* *Ranunculus acris* *Cheiranthus Cheiri*	No formation of starch (5 days). The leaves became unhealthy.
B. Solution applied to the Roots.		
Same solution......	*Acer pseudoplatanus* (3 plants) *Phaseolus vulgaris* (4 plants)	No formation of starch (6 days).

The plants were evidently injured by this solution, presenting a yellowish appearance at the end of six days; one of the plants of *Acer* and three of the *Phaseolus* failed to recover their normal growth when planted out, and ultimately died.

C. Solution applied to Surface of Leaf.

Solution used.	Plants.	Results.
Same solution......	*Tilia Europæa* (3 leaves) *Phaseolus vulgaris* (5 leaves)	No formation of starch (5 days). Leaves became yellow when the solution had been applied.

No. 1.—*Experiments with Acroleïn* (continued).

II. Acroleïn compounds—

(*a*.) "Acroleïn Ammonia."

A. On Cut Branches.

Solution used.	Plants.	Results.
The culture solution + ·5—1 per cent. acroleïn-ammonia	*Alisma plantago* *Tilia Europæa* *Acer pseudoplatanus* *Scrophularia aquatica*	No formation of starch (4 days). Leaves not visibly injured.
B. Solution supplied to Roots.		
Same solution as above (A)	*Cheiranthus Cheiri* *Quercus robur* *Campanula glomerata* (2 plants)	No formation of starch (6 days).

The plants were not apparently injured by this solution; on planting out they all resumed growth; the plants of *Campanula* had formed starch again after ten days (not tested earlier) after planting out.

C. Solution applied to Surface of Leaf.

Solution used.	Plants.	Results.
Same solution as above	*Acer pseudoplatanus* *Tilia Europæa*	No formation of starch.

No. 1.—*Experiments with Acroleïn* (continued).

II. Acroleïn compounds—

(*b.*) $NaHSO_3$ compound of Acroleïn.

A. On Cut Branches.

Solution used.	Plants.	Results.
The culture solution + about 2 per cent. of the crystal. $NaHSO_3$ comp.	*Phaseolus vulgaris* *Ranunculus acris* *Tilia Europæa*	Became unhealthy on 2nd day and were withered at end of 4 days. No starch.
B. Solution supplied to Roots.		
Same solution as above	*Phaseolus multiflorus* *Tilia Europæa* (2 plants) *Cheiranthus Cheiri* (2 plants)	All killed at end of 5 days; showed marked injury after 48 hours. No starch.

No. 1.—*Experiments with Acroleïn* (continued).

Experiments with Water Plants.

Method and apparatus as described on pp. 155—156.

Solution used.	Plants.	Results.
The culture solution diluted with an equal vol. of distilled water + 0·25 per cent. acroleïn	*Anacharis alsinastrum* *Fontinalis antipyretica* *Chara vulgaris*	*No* starch formed. Chlorophyll markedly injured after 24 hours. No bubbles of gas evolved in bright sunlight after the first 24 hours.
Three plants of *Anacharis* from different sources were used. Two " *Chara* " " "		

No. 2.—*Experiments with Allyl Alcohol* (C_3H_6O) ($CH_2{\cdot}CH{\cdot}CH_2OH$).

The allyl alcohol was prepared in the usual manner by distilling a mixture of glycerin and crystallised oxalic acid with a little ammonium chloride, and rectifying the crude distillate from potassium carbonate, solid potash, and finally from lime. The substance used was collected in a separate receiver (93—95° C.).

A. On Cut Branches.

Solution used.	Plant.	Results.
The culture solution + 0·5 per cent. allyl alcohol	*Tilia Europæa* *Cheiranthus Cheiri* *Ranunculus acris*	*No* starch formed. Leaves became yellow and flaccid after 6 days.
	B. Solution supplied to Roots.	
Same solution as above	*Acer pseudoplatanus* (3 plants) *Phaseolus vulgaris* (2 plants) (Sand culture). *Quercus robur* (2 plants)	*No* starch formed in leaves (6 days). Plants were decidedly injured.

On planting out after the experiment, two plants of *Acer*, both of *Phaseolus*, and one of *Quercus* failed to resume growth, and ultimately (3 weeks) died.

No. 2.—*Allyl Alcohol* (continued).

Experiments with Water Plants.

Method and apparatus described on pp. 155—156.

Solution used.	Plants.	Results.
A. The culture solution diluted with an equal volume of distilled water + 0·5 per cent. allyl alcohol	*Anacharis alsinastrum* *Chara vulgaris*	Plants injured after 12 hours, and yellow or brown after 48 hours. *No starch.*
B. The culture solution diluted with twice its volume of distilled water + 0·1 per cent. allyl alcohol.	*Callitriche aquatica*	*No* starch formed (7 days). Plant not injured to outward appearance.

Not tested whether plant formed starch again under normal conditions after experiment, as specimens used were accidentally mislaid in laboratory.

No. 3.—*Experiments with Glucose (Starch-sugar).*

Commercial "pure glucose," obtained from Messrs. Hopkin and Williams, of London, was used.

A. It is well known* that cut branches of plants, leaves, &c., form starch when supplied with solutions of glucose. I did not repeat these experiments.

B. Solutions supplied to the Roots.

(1.) Solution used.	Plants.	Results.
The culture solution + 1 per cent. of glucose	*Quercus robur* *Cheiranthus Cheiri* *Euphorbia helioscopia* *Phaseolus vulgaris* *Acer pseudoplatanus*	All contained starch at the end of 4 days.

(2.) Finding that starch was formed under these circumstances, I commenced a new series of experiments, in order to observe whether the plants were able to withdraw the whole of the glucose from solutions, and how long a time was required for the first formation of starch. As the young plants of *Cheiranthus* were growing most vigorously at this time, I used them for this purpose.

The first point was easily answered in the affirmative. Using the previously mentioned culture solution (containing 1 per cent. of glucose), such an amount was taken as to contain 3·57 grams of glucose (about 400 c.c.); six plants of *Cheiranthus Cheiri*, with their roots immersed in the solution, had completely absorbed all the glucose at the end of five or six days. In another similar experiment, I found that three plants of *Acer pseudoplatanus* absorbed 1·86 gram of glucose in eight days, but a fungus mycelium was beginning to form at the end of this time, which may have assisted in removing the glucose. It was proved that all the glucose had disappeared from the solutions by the usual methods of testing, viz., with Fehling's solution, &c., &c.

In regard to the second point, I never found any formation of starch to occur with less than ten hours' exposure to light; but it is obvious that experiments of this kind are of very little value, as it is not possible to determine to what extent the previous treatment to deprive tissues of starch has affected the normal assimilation processes.

* See Introduction.

As would be expected, those plants which had been deprived of starch by keeping in the dark were considerably longer showing starch formation in their leaves than those which had been brought into a similar condition under the bell-jars by the absence of carbon supply (CO_2), although plants as nearly as possible of the same age and size were originally selected, and in each case used for the cultures as soon as they were found to be completely free from starch.

A few of the most nearly comparable results as to the time required are given below.

Plants in Culture Solution + 1 per cent. Glucose.

Plant.	Starch first detected in leaves after the lapse of	
Cheiranthus Cheiri (deprived of starch by absence of CO_2), 5 plants, A, B, C	(A) 13 hours (continuous) (B) 11 ,, ,, (C) 13 ,, ,,	from time of placing in culture solution.
Cheiranthus Cheiri (deprived of starch by placing in dark), 2 plants, C″, D *Note.*—About 7 hours in dark between 1st and 2nd day.	(C) 13 hours 1st day; 8 hours 2nd day (D) 13 hours 1st day; 10 hours 2nd day	

No. 4. *Experiments with Aldehyde (Acetic).*

The aldehyde was purified in the usual way by saturating an ethereal solution of commercial aldehyde with gaseous ammonia collecting the crystals, and distillation with dilute H_2SO_4.

The aldehyde ammonia used was a portion of that obtained in above; the crystals would be pure.

A. On Cut Branches.

Solution used.	Plants.	Results.
Culture solution + 0·75 per cent. aldehyde	*Ranunculus acris* *Acer pseudoplatanus* *Scrophularia aquatica* *Alisma plantago*	No starch formed (5 days); leaves unhealthy towards end of experiments.
B. Solution supplied to Roots.		
Culture solution + 0·1 per cent. aldehyde (sand culture)	*Phaseolus vulgaris* *Phaseolus multiflorus* *Euphorbia helioscopia* *Cheiranthus Cheiri* (3 plants)	*No* starch formed (6 days).

On planting out at the end of experiment the plants of *Phaseolus vulgaris* and *P. multiflorus*, with two of the plants of *Cheiranthus Cheiri*, died,* but the others, although evidently injured, did not die within three weeks.

With "Aldehyde-ammonia."†

A. On Cut Branches.

Solution used.	Plants.	Results.
Culture solution + 1 per cent. aldehyde-ammonia	*Alisma plantago* *Ranunculus acris* *Tilia Europæa* *Lilium candidum*	*No* formation of starch (4 days).
	B. Supplied to Roots.	
Same solution as above	*Acer pseudoplatanus* (3 plants) *Phaseolus vulgaris* (2 plants) *Cheiranthus Cheiri* (4 plants)	*No* formation of starch (8 days).

All plants resumed growth on being planted out.

No. 5. *Experiments with Glycerin.*

Commercial "pure glycerin" was used.

A. Since A. Meyer‡ has shown that leaves supplied with glycerin do form starch, and E. Laurent§ has confirmed this observation, I did not repeat these experiments.

* Since the free aldehyde is a very volatile substance, giving off vapour at ordinary temperatures, it must be considered doubtful whether the injurious effect of the substance in the above experiment is to be attributed to action on the roots in solution, or to action of the vapour on the leaves.

A few drops of pure aldehyde allowed to evaporate under a receiver containing a plant of *Acer pseudoplatanus* quickly (24 hours) caused the death of the latter, as would be expected.

† The compound aldehyde-ammonia, = $CH_3COH{\cdot}NH_3$, is probably amidethylic alcohol, $CH_3CH{\cdot}NH_2{\cdot}OH$. The tendency of this body to undergo condensation changes with formation of basic nitrogen compounds is well known. (See 'Watts' Dict. of Chem.,' vol. 1, London, 1888; "Aldines and Aldehydines.")

‡ 'Botan. Zeitung,' 1886.

§ Laurent, in 'Botan. Zeitung,' 1886, p. 151.

B. Supplied to the Roots.

Solution used.	Plants.	Results.
The culture solution + 0·5 per cent. glycerin	*Phaseolus vulgaris* *Acer pseudoplatanus* *Quercus robur* *Campanula glomerata*	All formed starch after 5 days.

In a second series of experiments with the same solution as above I found that—

Cheiranthus Cheiri—	Had formed starch	
A. 2 plants........	after 48 hours	From time of placing in the culture solution.
B. 1 plant	„ 56 „	
C. 3 plants........	„ 60 „	
Acer pseudoplatanus—		
A. 1 plant	„ 68 „	
B. 3 plants........	„ 74 „	

For experiments with *Acer pseudoplatanus*, L. in solutions with varying amounts of glycerin, I found that no starch was formed when the solution was stronger than 10 per cent. glycerin,* and solutions 15—20 per cent. glycerin decidedly injured the plant in twelve hours, and ultimately caused its death.

The same results are obtained with other plants, *e.g.*, *Quercus robur* and *Euphorbia helioscopia*, as with *Acer pseudoplatanus*, L.

No. 6. *Experiments with Lævulinic Acid.*

The acid was prepared from the lævulose obtained by Kiliani's† process from commercial inulin.

The lævulose is boiled with dilute sulphuric acid, and the zinc salt of lævulinic acid obtained from the product by the process recommended by Grote and Tollens.‡

The ethyl ethereal salt was then obtained by decomposing the alcoholic solution of zinc salt with H_2S, filtering off the ZnS, boiling to expel H_2S, saturating with HCl, and distilling in the usual way.

* As A. Meyer states that leaves form starch when placed in 10 per cent. solutions of glycerin, it must be assumed that the root tissues are affected in these experiments, or that such strong solutions are unable to travel from the root to leaves.

† See Kiliani in 'Liebig's Annalen,' vol. 205, 1880; and also in 'Berichte Deutsch. Chem. Gesell.,' 1880, p. 2426.

‡ 'Liebig's Annalen,' vol. 175, 1875, and vol. 206, 1881.

The ethyl ethereal salt was saponified, and the barium salt obtained. From the barium salt the pure acid was obtained by decomposing with dilute H_2SO_4.*

The calcium salt used in No. 6, II, A. and B. was obtained by neutralising a portion of the pure free acid with $Ca(OH)_2$.

Experiments with "Lævulinic Acid."

A. With Cut Branches.

Solution used.	Plants.	Results.
The culture solution + 1 per cent. lævulinic acid	*Ranunculus acris* *Alisma plantago* *Scrophularia aquatica*	*No* starch formed (5 days); not apparently injured.
B. Solution supplied to the Roots.		
Same solution as above	*Phaseolus vulgaris* *Cheiranthus Cheiri* *Quercus robur* (3 plants)	*No* starch formed (8 days).
Plants all recovered normal growth on planting out, and had formed starch after 3 to 4 days.		
C. Solution placed on Leaves.		
Same solution as above	*Acer pseudoplatanus* (with roots in culture solution), 2 plants	*No* starch formed (5 days). Leaves not apparently injured where solution had been applied.

* Lævulinic acid obtained as described from lævulose or cane-sugar has been shown by Conrad ('Liebig's Annalen,' vol. 188, 1877) to be identical with β-acetyl-propionic acid (acetyl-propionic acid =

$$\begin{array}{l} CH_3\text{—}CO \\ | \\ CH_2 \\ | \\ CH_2 \\ | \\ COOH) \end{array}$$

obtained by the action of baryta-water on diethyl acetosuccinate. Lævulinic acid is therefore one of the "ketonic acids," which have been so largely used in recent chemical synthesis, and the non-formation of starch by the plants from this source I regard as particularly interesting.

It was my intention at the beginning of these experiments to try the calcium or magnesium salts of "aceto-acetic" acid and "acetyl-phenyl-propionic acid," as also some of the substituted "malonic ethers" of the form $R.R'.C.(CO.C_2H_5)_2$ (R.R'. = alcohol radicles), all of which are powerful reagents in organic synthesis, but, finding the results negative with lævulinic acid, I conclude that they would probably not be different with the above-mentioned bodies.

The Calcium Salt of Lævulinic acid.

A. On Cut Branches.

Solution used.	Plants.	Results.
The culture solution + 1 per cent. calcium lævulinate	*Ranunculus acris* *Scrophularia aquatica* *Alisma plantago*	*No* starch formed (10 days).
B. Solution supplied to the Roots.		
Same solution as above	*Cheiranthus Cheiri* *Quercus robur* *Acer pseudoplatanus*	No starch formed in the leaves (8 days).

Plants apparently uninjured, resumed growth, and had formed starch again (*Acer*) after 10 days from planting out.

No. 7. *Experiments with Saccharon* (*Cane-sugar*).

The saccharon used was pure cane-sugar obtained from Kahlbaum, of Berlin; it gave no reduction on heating with Fehling's solution at 100° for ten minutes. Many specimens of ordinary cane-sugar contain a considerable amount of glucose, and are obviously unsuitable for such investigations.

A. As A. Meyer and E. Laurent* have shown that starch is formed by leaves, cut branches, &c., placed in the solutions of cane-sugar, I did not repeat these experiments.

B. Solution supplied to the Roots.

Solution used.	Plants.	Results.
Culture solution + 0·5 per cent. saccharon	*Acer pseudoplatanus* *Cheiranthus Cheiri* (3 plants) *Phaseolus vulgaris* (2 plants) *Euphorbia helioscopia*	Starch was formed at end of 4 days in all the leaves.

Wishing to determine whether saccharon is as readily absorbed by the roots of plants as glucose (compare No. 3, p. 162), I selected six young plants of *Cheiranthus Cheiri*, as nearly as possible of equal size, so that three of them were about the same weight as the other three (a = three plants, B = three nearly similar plants; B weighed 0·01 gram more than a); (a) were placed in a cylinder containing 100 c.c. of the culture solution + 0·5 glucose; B in a similar

* *Loc. cit.*

cylinder containing 100 c.c. of the culture solution + 0·5 gram saccharon.

The two cylinders were placed under the same bell-jar and arranged so as to be as nearly as possible illuminated to the same extent.

After three days (the leaves of (*a*) and (B) then containing starch) I determined the remaining glucose and saccharon in the cylinders by making up to an equal volume in each case and withdrawing an aliquot part for analysis.

Of the glucose 0·237 gram remained (absorbed 0·263 gram).

Of the saccharon 0·302 gram remained (absorbed 0·198 gram).

This experiment was confirmed by arranging another six plants in a similar manner and testing each day how much of the glucose and saccharon had been removed during the preceding twenty-four hours.

Commencing on the second day after placing the cylinders in the bell-jar, it was found at the beginning of the third day that—

(2nd to 3rd day), in 24 hours, 0·085 gram glucose, 0·061 gram saccharon.

(3rd to 4th day), in 24 hours, 0·074 gram glucose, 0·062 gram saccharon.

I therefore conclude that glucose is more readily taken up by the roots of plants (from 5 per cent. solutions) than saccharon.*

No. 8. *Experiments with "Dextrins."*

The dextrins used were of two kinds:—1. Erythro-dextrin; 2. Achroo-dextrin.

Erythro-dextrin used was obtained as "dextrin" in dry state from Messrs. Hopkin and Williams, of London. It was well washed with strong alcohol before solution in water, to remove any traces of glucose.

The achroo-dextrin was prepared from the above by heating with 5 per cent. H_2SO_4 on a "calcium chloride bath" (temperature above 100°) till the solution, neutralised with $BaCO_3$, gave no trace of reaction with I in KI.

The dextrin was then precipitated by the addition of strong (95 per cent.) alcohol and washed with the same until the washings were perfectly free from "reducing" substances, dried, and dissolved in water.

* This result is not in keeping with the experiences of A. Meyer as to the relative value of glucose and saccharon. When leaves are placed in 10 per cent. solutions he finds starch to be more readily formed from saccharon than dextrose (glucose). I have not experimented with "lævulose."

1. Erythro-dextrin.

A. With Cut Branches.

Solution used.	Plants.	Results.
The culture solution + 1 p. c. erythro-dextrin	*Acer pseudoplatanus*	No starch formed in the leaves (5 days).
	Tilia Europæa	Same as above } no starch.
	Phaseolus multiflorus	Same as above } no starch.

2. Achroo-dextrin.

A. With Cut Branches.

Solution used.	Plants.	Results.
The culture solution + 1·5 p. c. achroo-dextrin	Same plants as above (Erythro-dextrin)	*No* starch formed (5 days).
B. Supplied to Roots.		
Same as 2A above ..	*Epilobium hirsutum*	*No* starch formed in the leaves (5 days).
	Tilia Europæa	
(Sand culture)	*Cheiranthus Cheiri*	*No* starch formed in leaves (5 days).

All plants resumed growth and formed starch again when planted out.

No. 9. *Experiments with Inulin.*

Commercial inulin, obtained from Messrs. Hopkin and Williams, of London, was used. It was thoroughly washed with strong alcohol (to remove any glucoses) and dried *in vacuo* before solution in water. The solutions were used immediately after preparation.*

A. A. Meyer† has shown that leaves do form starch from solutions of inulin. I did not repeat this experiment.

* This experiment with inulin I regard as inconclusive, because it is not probable that the substance used was pure. Solutions of inulin on standing for any length of time I have always found to contain lævulose, from which the starch detected might have been produced.

Kiliani ('Liebig's Annalen,' vol. 205, 1880) has pointed out the very great difficulty of obtaining pure inulin (compare also J. R. Green, 'Annals of Botany,' vol. 1, No. III).

† *Loc. cit.*

B. Solution supplied to the Roots.

Solution used.	Plants.	Results.
The culture solution + 1 per cent. inulin	*Acer pseudoplatanus* *Cheiranthus Cheiri*	Starch was formed in the leaves at the end of 5 days (not tested earlier).

No. 10. *Experiments with "Soluble Starch."*

The starch solution was prepared by pouring starch (wheat-starch) rubbed into a thin paste with water (cold) into an excess of boiling water and boiled for five minutes ; on cooling the solution was filtered through paper and diluted till the strength was about 1 per cent.*

A. With Cut Branches.

Solution used.	Plants.	Results.
The culture solution + about 1 per cent. starch (soluble)	*Acer pseudoplatanus*	Starch grains formed in the leaves after 24 hours, abundant after 48 hours.
	Tilia Europæa	Ditto after 48 hours ; not so abundant as above ; increased after 3 days.

B. Supplied to Roots.

Solution used.	Plants.	Results.
(a.) Same solution as in 10 A	*Acer pseudoplatanus*	No starch formed [illegible] days).
	Epilobium hirsutum†	Ditto.
	Phaseolus multiflorus	Ditto.
	Tilia Europæa	

* Sachs' method being obviously inapplicable in this case, micro-chemical observations on sections were used to detect the starch grains in the leaves.

† The two seedlings of *E. hirsutum* used were apparently injured in a[illegible]ing the additional soluble starch, as they withered during the second six days. As the results were negative in other cases, this experiment was not repeated.

Except in case of *E. hirsutum* (see note above), the plants all resume[illegible]wth and formed starch after several days when planted out.

(b.) More "soluble starch" added to the culture solution in each of the above till solution contained about 7 per cent. "soluble starch," and experiments continued for another six days with same plants.

Same plants. No starch formed.

No. 11. *Experiments with Glycogen.*

An ordinary solution of glycogen obtained by extracting the liver of a freshly-killed rabbit was purified by Cl. Bernard's* method, in which the glycogen is first precipitated by alcohol and the well-washed precipitate (with alcohol) dissolved in strong potash and boiled for half an hour. The solution is then diluted, filtered, and again precipitated in alcohol, well washed with the same, and the precipitate dissolved in water. The aqueous solution is strongly acidified with acetic acid (to render the insoluble (in alcohol) potassium carbonate soluble as potassium acetate), finally precipitated with alcohol, well washed with the same, and dried at a low temperature. If kept for any length of time after the preparation, the dry powder was thoroughly washed with alcohol and again dried before making the solutions used in the experiments.

The solutions remained opalescent and did not contain any "reducing substances" after the experiments; this was proved by precipitating the glycogen, &c., with strong alcohol and testing the filtrate after evaporating off the alcohol, &c., &c., in the usual way.

No. 11.—*Experiments with Glycogen.*

A. With Cut Branches.

·lution used.	Plants.	Results.
lture solution ·5 per cent. ·ogen	*Alisma plantago* *Ranunculus acris* *Epilobium hirsutum*	*No* starch formed (6 days). Leaves not apparently injured.
	B. Solution supplied to Roots.	
The culture solution + 1 per cent. glycogen.	*Acer pseudoplatanus* *Phaseolus vulgaris* (2 plants) *Cheiranthus Cheiri* (4 plants)	*No* starch in the leaves at end of 6 days.

The plants were not apparently injured, and resumed growth on being ·lanted out.

* Hoppe-Seyler, 'Traité d'Analyse Chimique appliquée à la Physiologie. P..., ·87 (Translation from German), pp. 147—148.

No. 12. *Experiments with "Extract of Natural Humus."*

An extract of "humus" was obtained by digesting the soil—a light leaf mould—with dilute alcohol on a water-bath for eight hours and filtering through asbestos and powdered glass. The alcohol was distilled off on a water-bath, the residual solution diluted with water, and again filtered as above. 100 c.c. of this solution evaporated to dryness at 110° C. left 0·3708 grain of solid residue, which showed on combustion that it contained 15 per cent. of carbon.*

As bacteria, fungi, algæ, &c., rapidly make their appearance when such a solution is allowed to stand, I found it convenient to keep the alcoholic extract, referred to above, and evaporate off the alcohol just before use.

No. 12.—*Experiments with "Extract of Natural Humus."*

A. With Cut Branches.

Solution used.	Plants.	Results.
I. Culture solution + 20 c.c. of the "extract"	*Scrophularia aquatica* *Tilia Europæa* *Phaseolus vulgaris*	*No* formation of starch (6 days).
II. Ditto	*Cheiranthus Cheiri*	*No* starch (8 days).
	B. Solution supplied to the Roots.	
Same solution as above	*Acer pseudoplatanus* (2 plants) *Quercus robur* (2 plants) *Phaseolus vulgaris* (1 plant) *Cheiranthus Cheiri*	Starch *was* formed in the leaves, but in all cases only a small quantity after 6 days (not tested earlier).

* The residue contains a small quantity of nitrogen, but the amount varies greatly in different extracts. I have not experimented with any extract of "natural" humus perfectly free from N (compare Exp. No. 13).

"Humus Extract."

Experiments with Water Plants.

(Method and apparatus as described on pp. 155—156).

Solution used.	Plants.	Results.
A. The culture solution diluted with twice its volume of distilled water + "humus extract"	*Sparganium natans* (parts of 4 plants) *Callitriche aquatica*	*No* starch formed. Plants not injured after 10 days. No bubbles of gas evolved in bright sunlight after six hours.
B. Some humus-containing soil which had been heated to low red-heat (10 hours) in a "muffle" and cooled in a desiccator added to above solution just before transferring the plant to solution	*Callitriche aquatica*	Ditto ditto

No. 13. *Experiments with the "Humus-like" product of Alkalies on Cane-Sugar.**

Cane-sugar (saccharon)† was boiled with strong solution of KOH for half an hour, and the whole solution diluted with water and then acidified with HCl, which causes the separation of flocculent brown "humus-like" substances. The precipitate was washed with dilute acid, dried, and then boiled with water; the aqueous extract so prepared was used in these experiments after filtering through paper.

The above aqueous extract contained about 2 per cent. of solid substances.

* The chemical nature of these brown "humus-like" substances is very little known, but they are generally assumed by chemists to be closely related to the chief constituents of natural "humus," which must be chiefly derived from the decomposition of cellulose and ligneous matter. (Compare Beilstein, 'Handb. d. Org. Chem.,' 2nd Edition.—"Huminsubstanz.")

† Compare Conrad and Guthzeit, in 'Berichte Deutsch. Chem. Gesell.,' 1885, p. 439.

No. 13.—*Experiments with the "Humus like" Product of Alkalies on Cane-Sugar.**

A. With Cut Branches.

Solution used.	Plants.	Results.
The culture solution + 25 c.c. of the aqueous extract	*Tilia Europæa* *Phaseolus vulgaris* *Euphorbia helioscopia*	*No* starch formed (6 days).
B. Solution supplied to the Roots.		
Same solution as above	*Cheiranthus Cheiri* (3 plants) *Acer pseudoplatanus* (1 plant) *Quercus robur*	*No* formation of starch in the leaves (8 days).

The plants resumed growth on being again planted out after the experiment.

Abstract of Results of Experiments.

1. Starch formed when compound is supplied either direct to leaves or to roots, with—

Glucose,
Glycerin,
Saccharon,
Inulin. } (Observed by A. Meyer for "supplied to shoots"). Experiments on shoots not repeated.

2. Starch formed when the compound is supplied direct to leaves but *not* when supplied direct to roots, with—

Soluble Starch.

3. Starch formed when the compound is supplied to the roots, but *not* when supplied direct to the leaves, with—

"*Humus Extract.*"

4. Starch *not* formed at all—with acroleïn, or compounds; allyl alcohol; dextrin; glycogen; aldehyde or compounds; lævulinic acid; artificial humus substance.

5. Glucose more readily taken up by roots from 0·5 per cent. solution than saccharon. All the glucose can be withdrawn from a 1 per cent. solution by roots if left in the solution sufficiently long.

I conclude from these experiments—

* When 5 per cent. of glucose was added to a portion of the aqueous extract, and the beaker containing this mixture exposed to light, a considerable amount of fungus mycelium (? bacteria) formed in the solution by the end of 14 days.

That green plants cannot normally obtain carbon for "assimilation" from any substances except carbohydrates or bodies closely related to them; not from aldehydes or their derivatives, and not from all carbohydrates even.

That a compound may be a source of carbon when supplied to the leaves, but not when supplied to the roots, and *vice versâ.*

That (since parasitic and saprophytic plants, and especially fungi, undoubtedly do always obtain their carbon from complex organic substances) green plants, owing to the normal process of obtaining carbon being from CO_2, have to a large extent lost the power of using such substances as a source of carbon.

That many green plants (? all) behave in the same manner towards such substances.

[Contrast fungi, which often are characterised by decomposing special substances.]

That (since neither leaves nor roots can avail themselves of carbon in the form of aldehyde or its compounds—formose, allyl alcohol, acroleïn, lævulinic acid, &c.) it is still uncertain whether or not a single substance of an aldehydic or ketonic nature is really formed by plants as an intermediate product between CO_2 and H_2O and glucose (or starch); but, if such is produced, it can only be polymerised by the plant under special conditions,* probably at the moment of formation.

* Compare Loew, 'Berichte Deutsch. Chem. Gesell.,' 1889, p. 470.

February 6, 1890.

Mr. JOHN EVANS, D.C.L., Treasurer and Vice-President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "A New Theory of Colour-blindness and Colour-perception." By F. W. EDRIDGE-GREEN, M.D. Communicated by Dr. LAUDER BRUNTON, F.R.S. Received January 28, 1890.

II. "Memoir on the Symmetrical Functions of the Roots of Systems of Equations." By Major P. A. MACMAHON, Royal Artillery. Communicated by Professor GREENHILL, F.R.S. Received January 30, 1890.

(Abstract.)

The object of the present memoir is the extension to systems of algebraical quantities of the new theory of symmetric functions which has been developed by the author in regard to a single system in Volume 11 and succeeding volumes of the 'American Journal of Mathematics.' In the theory of the single system the conceptions and symbolism are to a large extent arithmetical, and are based upon the properties of single integral numbers and their partitions into single integral parts. In this sense the theory may be regarded as being unipartite. In the present generalisation to the case of m systems of quantities the fundamental ideas proceed, not from a single number, but from a collection of m single numbers. In regard to number, weight, degree, part, and suffix, the collection of m numbers invariably replaces the single number of the theory of the single system. In this view the theory of the m systems is m-partite.

The quantities, to which the symmetric functions relate, may be regarded as the solutions common to m non-homogeneous equations each in m variables. Schläfli, in the 'Vienna Transactions' (*Denkschriften*) for 1852, added another linear non-homogeneous equation in m variables, and then forming the eliminant of the $m + 1$ equations,

thereby obtained an identity which is fundamental in the subject. This identity involves those symmetric functions which are here termed fundamental, and marks the starting point of the present investigation.

The memoir is divided into sixteen sections. In § 2 a preliminary algebraic theory is given, and then in § 3 is commenced the theory of the differential operations. A prominent feature presents itself in the very interesting correspondence between the algebras of quantity and differential operation.

In § 4 is discussed the theory of three identities, formed similarly to the fundamental identity alluded to above, and such that the quantities involved are related in a particular manner. The theory of differential operation proceeds collaterally with that of quantity. The succeeding four sections, § 5–§ 8, are devoted to the results which flow in a direct manner from this discussion. In particular, three distinct laws of symmetry are established, large generalisations of those established by the author in the 'American Journal of Mathematics' (*loc. cit.*). Of these the first two are of importance, and are examined in detail. A leading idea in these theorems, as in the whole investigation, is the "separation" of a partition; the separation bears the same relation to the partition as the partition to the number or collection of numbers. The first law of symmetry appears to be of cardinal rank in symmetrical algebra. It involves, at sight, a law of expressibility in the theory of separations which is of a general character. It demonstrates at once the possibility of forming a pair of symmetrical tables of symmetric functions in connexion with every partition of every collection of m numbers (regard being paid as well to order as to magnitude). The necessary tables for the bipartite theory (*i.e.*, of two systems) as far as the weight, four inclusive, are exhibited in § 14. An extension of the Vandermonde-Waring law for the expression of the sums of the powers of the roots of an equation by means of the coefficients is generalised, in a single formula, from two points of view in § 6. In § 9 and § 10 the decomposition is effected of the operations previously encountered in § 3. The linear weight operations are found to break up into as many linear partition operations as the weight possesses partitions. An important theorem is reached when it is established that the annihilation of a symmetric function by a linear weight operation necessitates annihilation by each partition operation of the same weight. The weight operations of higher orders, partially examined in § 3, which may be termed "obliterating," from their characteristic property, break up similarly into partition operations which possess an obliterating property in regard to products of symmetric functions. In this manner all the differential operations of § 3 are adapted for use in the theory of *separations*, as distinct from the theory ordinarily

considered, which is in fact that of fundamental or single-unitary symmetric functions.

These partition operations possess an algebra also in exact correspondence with the algebra of quantity.

In § 10 the partition obliterating operations are applied to the theory of multiplication.

In § 12 a transformation is established by means of which functions of differences can, with special exceptions, be converted into non-single-unitary symmetric functions. This theorem is the analogue of the transformation of the theory of invariants first given by the author in Vol. 6 of the 'American Journal of Mathematics.'

§ 15 proves a useful law to which the tabular numbers are subject, connected with the idea of grouping the separations together in a particular manner.

In conclusion the memoir consolidates and largely generalises the author's recent researches alluded to above at the beginning of the Abstract.

Presents, February 6, 1890.

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February 13, 1890.

Sir G. GABRIEL STOKES, Bart., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "The Liquation of Gold and Platinum Alloys." By EDWARD MATTHEY, F.S.A., F.C.S., Associate Royal School of Mines. Communicated by the President. Received January 17, 1890.

It is a well known fact that when molten alloys of certain metals are cooled, some of the constituents separate and become concentrated either in the centre or in the external portions of the solidified mass; to this segregation the name of liquation is given. It is specially noticeable in the case of silver-copper alloys, and its importance is now being widely recognised in almost all branches of metallurgy.

In the case of gold, however, the phenomenon of liquation does not appear to have been much observed. Gold alloys, to the value of many millions sterling, pass annually from hand to hand upon the results of assays cut from the external portions of ingots, which assays cannot, of course, be trustworthy, if the centre of the bars differs in composition from the external portions. Peligot has recently endeavoured to obtain evidence of liquation in gold-copper alloys, and has concluded that it does not exist.* Roberts-Austen,† who has devoted much time to the study of liquation, has also satisfied himself that gold-silver alloys do not rearrange themselves on cooling.‡

It is, of course, well known that gold does not retain on solidifying certain metals of the platinum group; for instance, iridium, when associated with it, always tends to fall through the fluid metal, and is found at the bottom of the solidified mass, but this is probably not a case of true rejection of a metal by liquation, but is due to the higher specific gravity of the iridium, coupled with the fact that the usual heat at which gold is melted is not sufficiently high to bring

* 'Bulletin Société d'Encouragement,' 1889, p. 481.

† 'Roy. Soc. Proc.,' vol. 23, 1874, p. 481.

‡ 'Nineteenth Annual Report of the Mint,' 1888, p. 35.

about a true alloy. It appeared to me that alloys of gold and platinum would well repay examination. They have been generally considered to be uniform in composition, but certain results which I obtained in the course of their treatment led me to suspect that they would give interesting results, and the following experiments were therefore undertaken.

The metal platinum frequently occurs in the gold and silver bullion which has to be treated by the ordinary methods of refining, and its presence occasions no small amount of trouble to the refiner.

It is well known that there are two methods of refining, both of which involve alloying one part of gold with (about) three parts by weight of silver, and treating the mass directly—

(*a.*) With nitric acid,
(*b.*) With sulphuric acid.

The final result from either method, if properly conducted, is fine gold and fine silver—that is to say, if the alloy so treated is composed of gold and silver only (a little copper present making no difference).

In the case of platinum being present in the gold or the silver, if it is refined by the nitric acid process, the platinum, when existing in small proportions, is eliminated with the silver, becoming dissolved up with it, leaving the gold free, and the platinum so dissolved can afterwards be readily separated from the silver, but upon the large scale refining by means of nitric acid is far too costly; practically, therefore, this has to be replaced by the sulphuric acid process.

In an alloy of gold and silver, containing a small proportion of platinum, nearly all the silver is dissolved by the sulphuric acid, leaving the platinum associated with the gold.

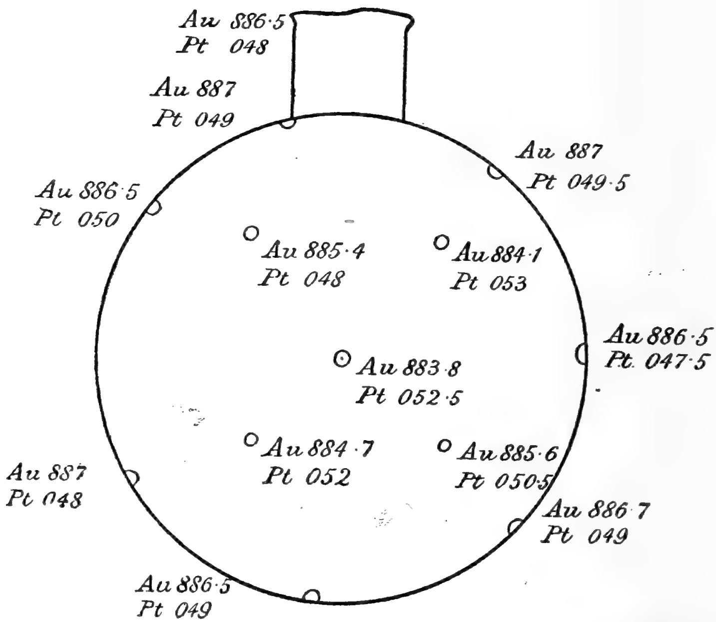
In order to simplify matters for further treatment, this partially refined gold holding the platinum is melted and assayed, to determine the amount of platinum and gold it contains; it is the platinum-gold alloys so obtained that I desire to bring under notice.

It has been found in practice that the ordinary method of assaying a small portion cut from one end of a bar or ingot of such metal does *not* indicate the actual percentage of gold and of platinum existing in the entire mass, and it is therefore evident that the platinum has been redistributed by liquation during the cooling and solidification of the mass.

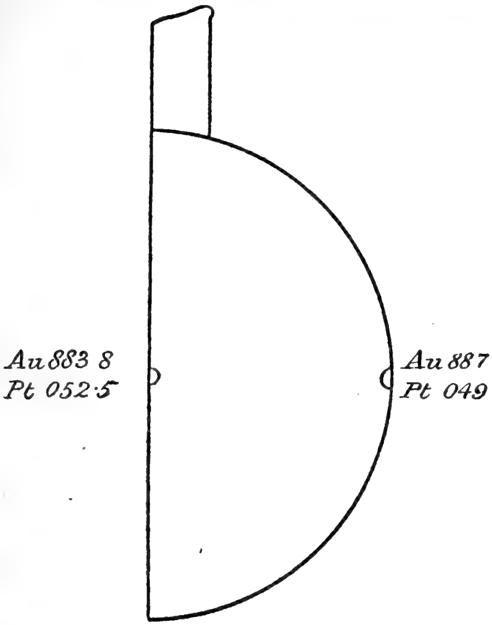
Having been struck by the experiments made by Professor Roberts-Austen, as detailed in the paper to which reference has already been made, I cast some gold containing platinum into a special iron mould 3 inches in diameter, and cut the spheres of metal so obtained in two halves. I may mention that I had to cast these spheres many times over in order to obtain a solid casting, so great was the shrinkage.

Portions were then carefully taken from each of the points marked on the diagrams A, B, and C given herewith, and the results of the assays of the metal taken at each point of the hemispheres are indicated on the diagrams.

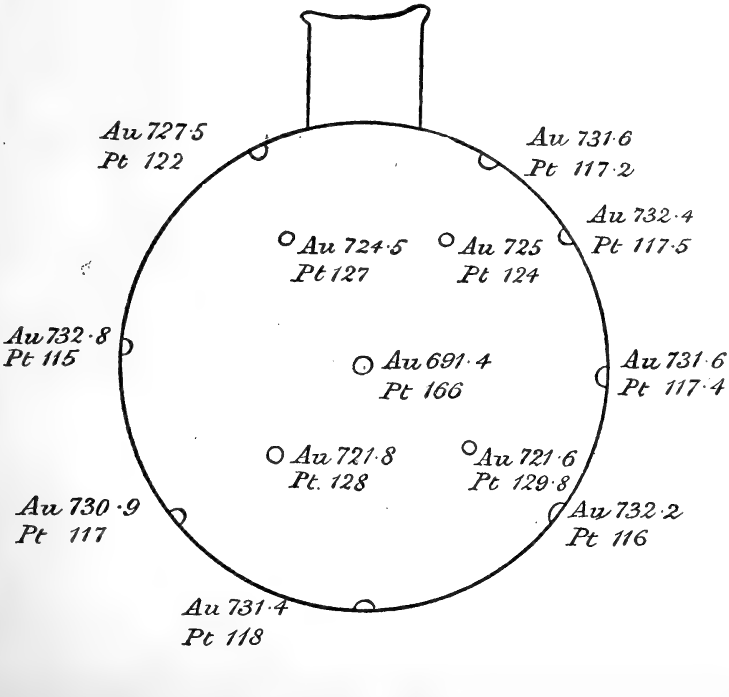
A.



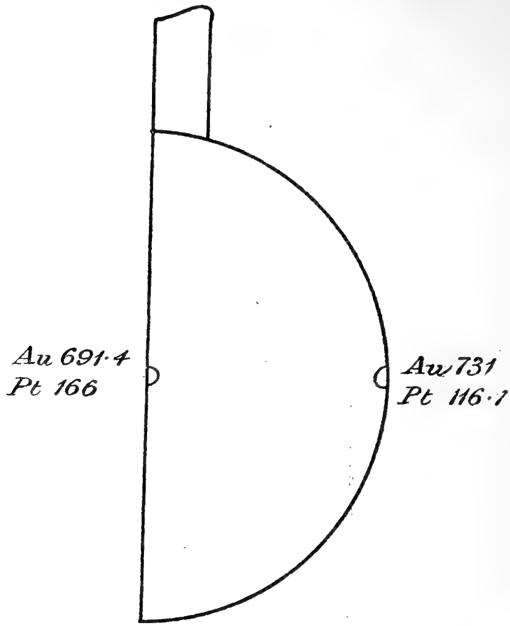
A.


Au 883 8
Pt 052·5
Au 887
Pt 049

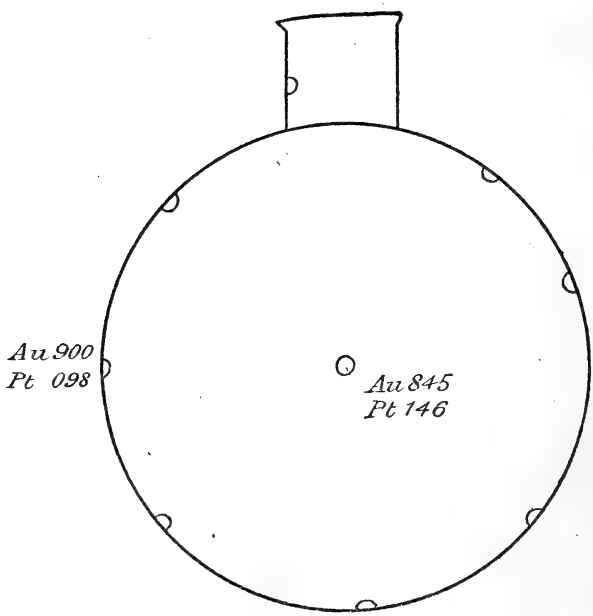
B.


Au 727·5
Pt 122
Au 731·6
Pt 117·2
Au 732·4
Pt 117·5
Au 724·5
Pt 127
Au 725
Pt 124
Au 732·8
Pt 115
Au 691·4
Pt 166
Au 731·6
Pt 117·4
Au 721·8
Pt. 128
Au 721·6
Pt 129·8
Au 730·9
Pt 117
Au 732·2
Pt 116
Au 731·4
Pt 118

B.



C.



A. Composed of about 880 gold to 050 platina.

B. Composed of about 700 gold to 120 platina.

In the one case the maximum difference between the gold percentage is a variation of 032, viz., 887 on the outside against 883·8 at the centre of the alloy, and in the platinum 047·5 on the outside against 052·5 at the centre, an extreme variation of 005 is shown.

In the other case the maximum difference between the gold percentage is a variation of 041, viz., 732·4 on the outside, against 694·1 at the centre of the alloy, and in the platinum 122 on the outside against 166 at the centre, an extreme variation of 044.

Thus showing indisputably that *the platinum in cooling liquates from the gold and becomes concentrated towards the centre of the alloy.*

In support of these experimental results I give the actual figures obtained from six platinum-gold ingots, taken at different times and of different qualities, as they occurred in the course of refining commercially. Each of these bars, after melting and assaying, was separately heated with a view to extract the amount of gold contained. It will be at once seen that the higher percentage of gold indicated by the assay of a portion cut from one end of the ingot is *not* borne out by the actual amount of fine gold obtained by refining, which, of course, truly represents the proportion of gold existing in each bar.

Number.	Weight in troy ounces.	Platinum by assay.	Gold by assay.	Percentage of gold by the fine gold actually obtained.
42	728·5	0·111	0·825	0·812
67	355·0	0·120	0·660	0·630
109	589·5	0·120	0·800	0·780
126	435·0	0·045	0·850	0·845
149	480·5	0·086	0·842	0·830
188	473·0	0·110	0·830	0·821

These results prove that the percentage of gold in the outer portion of ingots of platinum-gold alloy does not represent the true percentage of gold in the alloy, and that liquation *does take place* to an extent which, independently of its scientific and metallurgical interest, has, I believe, been by many overlooked up to the present time in commercial transactions with such metal.

The results given were observed in platinum-gold alloyed with silver, with copper, and with both silver and copper; but, in order to prove whether or not such alloy had any tendency to carry the

platinum to the centre of the mass, I melted 900 parts of fine gold with 100 parts of pure platinum, and, after repeated meltings, cast this alloy into the same mould used for the experiments recorded above. The result was, as in the previous cases, liquation of the platinum towards the centre of the sphere, the gold and platinum in 1000 parts being as 900 to 098 on the exterior, against 845 and 146 at the centre of the mass (see diagram C).

II. "On the Unit of Length of a Standard Scale by Sir George Shuckburgh, appertaining to the Royal Society." By General J. T. WALKER, R.E., F.R.S. Received February 3, 1890.

In the determinations of the length of the seconds pendulum, which were made in London by Kater and at Greenwich by Sabine, and are described in the 'Philosophical Transactions' for 1818, 1829, and 1831, the distance between the upper and lower edges of the pendulum was measured off on a standard scale which had been constructed by Sir George Shuckburgh. The scale had not been compared with any of the modern standard scales, but it had been preserved with much care with the instruments appertaining to the Royal Society.

In the autumn of 1888, M. le Commandant Defforges, an officer of the French Geodetic Survey, came to England to take a share in operations for the determination of the difference in longitude between Greenwich and Paris, and also to determine the length of a French seconds pendulum at Greenwich. He kindly undertook to comply with a suggestion which was made to him by me, to compare the portion of Shuckburgh's scale which had been employed by Kater and Sabine with one of the standard metre bars of the International Bureau of Weights and Measures in Paris. The Council of the Royal Society assented, and the scale was sent across to Paris and brought back again by special messenger.

The details and results of the comparison are given in the following account by Commandant Defforges, from which it will be seen that the scale was compared with the French metrical brass scale, N, at the temperature of 48·7° F., at which the distance between Kater and Sabine's divisions, 0 and 39·4, of the Shuckburgh scale was found equal to 1·0006245 metre. On reducing to the temperature of 62° F., which was employed by Kater and Sabine, this distance becomes 1·0007619 metre, which is equivalent to 39·400428 inches if we adopt the relation 1 metre = 39·370432 inches, which was determined by Colonel Clarke, C.B., of the Ordnance Survey, and is given in his valuable work on the Comparisons of Standards of Length. Thus

the actual length of the space 0 to 39·4 on the Shuckburgh scale may be regarded with some probability as differing by not more than about 0·0004 inch, or, say, the 100,000th part, from the quantity which the scale indicates.

Comparaison exécutée au Bureau International des Poids et Mesures entre la Règle en laiton de la Société Royale dite "Règle de Kater" et le Mètre international.

La règle en laiton de la Société Royale a été disposée le 4 Décembre, 1888, dans le comparateur universel de Starke et Kammerer pour y être comparée avec la règle N du Bureau International des Poids et Mesures. La règle N est une règle en laiton.

La règle de la Société Royale était posée à plat sur le banc nivelé du comparateur pour se rapprocher le plus possible de sa situation pendant les expériences de Kater. Les mesures ont été faites le 5 Décembre, 1888, à la température ambiante de 9·3° (48·7° F.), par M. le Docteur Benoît, aujourd'hui Directeur du Bureau International, et M. le Commandant Defforges, chacun des observateurs exécutant l'un après l'autre les pointés aux deux microscopes du comparateur.

On a comparé, pour satisfaire au désir de M. le Général J. T. Walker, l'intervalle 0, 39·4 p. de la règle de Kater au mètre étalon N. En désignant par S la règle anglaise, on a trouvé, par une série de mesures très concordantes :—

S—N.

	Température.	Observateur Benoît.	Observateur Defforges.
	9·284° C.	$+412{\cdot}6\mu$	$+413{\cdot}0\mu$
	9·347°	$+411{\cdot}2\mu$	$+413{\cdot}0\mu$
Moyenne..	9·315°	$+412{\cdot}4\mu$.	

Donc, en moyenne,

$$\text{à } 9{\cdot}315^\circ \quad S_{(0,\,39{\cdot}4\,\text{p.})} = N + 412{\cdot}4\mu.$$

Or, d'après les déterminations antérieures de la règle N, l'une des règles les mieux étudiées du Bureau International, on a, à 9·315°,

$$N = 1\,\text{m.} + 212{\cdot}1\mu.$$

Donc, toujours à la température de 9·315°,

$$S = 1\,\text{m.} + 624{\cdot}5\mu.$$

Donc, en résumé :

$$S_{[0,\,39{\cdot}4]\,9{\cdot}315^\circ\,\text{C.}} = 1{\cdot}0006245\,\text{m.}$$

Les deux observateurs ont en outre mesuré la valeur de un dizième de pouce entre les divisions 39 pouces et 40 pouces, ils ont trouvé :

$$\tfrac{1}{10} \text{ de pouce} \quad S = 2532{\cdot}6\mu.$$

Calcul, en mètres, de la Longueur du Pendule simple à Londres d'après Kater.

D'après le mémoire de Kater, la valeur de la distance entre les couteaux du pendule convertible mesurée à l'aide des contacts Aa, Bb était égale à l'intervalle (0, 39·4 pouces) de l'étalon augmenté de

$$\left.\begin{array}{ll} \text{1ère mesure,} & 956{\cdot}47 \\ \text{3me} \quad ,, & 955{\cdot}65 \end{array}\right\} 956{\cdot}06 \text{ divisions du micromètre.}$$

La même longueur, mesurée directement entre les arêtes des couteaux (couteaux noirs sur fond blanc), était égale à l'intervalle (0, 39·4 p.) augmenté de

960·00 divisions du micromètre.

Négligeant la correction d'irradiation admise par Kater, et qui n'aurait pas dû être appliquée à cette dernière mesure (voir Defforges, 'Intensité absolue de la Pesanteur,' page 47), et prenant la moyenne des longueurs obtenues par les deux méthodes, on aurait, pour la distance des arêtes, d'après Kater

Intervalle, [0, 39·4 p.] + 958·03 divisions,
à 62° Fahrenheit.

Or, la comparaison de Breteuil ayant été faite à 9·315° C. ou 48·8° F., il faut utiliser le coefficient de dilatation du pendule donné par Kater et qu'il paraît supposer égal à celui de la règle étalon pour ramener les résultats de la comparaison de Breteuil à 62° F. On trouve ainsi :—

$$S_{[0,\ 39\cdot4]\ 62^\circ \text{F.}} = 1{\cdot}0006245 \text{ m.}\{1 + 13{\cdot}8^\circ \text{ F.} \times 0{\cdot}000009959\}$$

$$= 1{\cdot}0007619 \text{ m.},$$

et

$$\frac{S(39{\cdot}0,\ 40{\cdot}0)}{10} = 2533\mu.$$

Appliquant ces valeurs à la distance donnée par Kater, on a :

$$958{\cdot}03 \text{ div.} \times \frac{2533}{2336{\cdot}3} = 1038{\cdot}7\mu,$$

et la distance des arêtes serait, à 62° F., en arrondissant le chiffre des microns :

$$1{\cdot}001801 \text{ m.}$$

G. Defforges.

[*Note.*—Since the above was in type I have been favoured by Mr. O. H. Tittmann with a copy of the U. S. Coast and Geodetic Survey's 'Bulletin,' No. 9, dated 15th June, 1889, on the relation of the yard to the metre, in which it is shown that the value 1 metre = 39·36980 inches is somewhat more probable than the value above adopted from Col. Clarke. This value makes the distance between divisions 0 and 39·4 of the Shuckburgh scale = 39·399796 inches, showing an error of −·0002 instead of +·0004 inches, as above indicated.—March 24, 1890.—J. T. W.]

III. "Note on the Spectrum of the Nebula of Orion." By J. Norman Lockyer, F.R.S. Received February 13, 1890.

[Publication deferred.]

IV. "Preliminary Note on Photographs of the Spectrum of the Nebula in Orion." By J. Norman Lockyer, F.R.S. Received February 13, 1890.

[Publication deferred.]

Presents, February 13, 1890.

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February 20, 1890.

Sir G. GABRIEL STOKES, Bart., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "A comparative Study of Natural and Artificial Digestions." (Preliminary Account.) By A. SHERIDAN LEA, Sc.D., Fellow of Gonville and Caius College, Cambridge, University Lecturer in Physiology, Cambridge. Communicated by Professor M. FOSTER, Sec. R.S. (From the Physiological Laboratory, Cambridge.) Received February 12, 1890.

When the conditions under which artificial digestions are usually carried on are compared with those under which digestion takes place in the alimentary canal, it is seen at once how imperfect the former are in comparison with the latter. The most important factors present in natural digestion, and wanting in the artificial, are: 1. Constant motion and mixing of the digesting mass. 2. Constant removal of the digestive products. 3. Continuous addition of fresh portions of digestive fluid. These factors must determine that natural digestion is more rapid and complete than is the artificial imitation, and my experiments were begun with a view to carrying out artificial digestions under conditions which should supply some at least of those which are usually wanting in such cases, but are always present in the natural process. The apparatus employed may be described as follows:—A stream of water is warmed by passing through a heated copper coil, and allowed to flow through a vertical cylindrical vessel in which is concentrically immersed a smaller cylindrical glass vessel some 2 feet high and 4 inches in diameter. By this means the contents of the inner vessel can be kept constantly at a temperature of 40° C. The lower end of the inner vessel is connected with a tube which passes through the side of the outer vessel, so that its contents may be drawn off and renewed whenever it may be desired to do so. The material to be digested is placed in a U-shaped loop of parchment-paper tubing, together with the digestive fluid, and this loop is then allowed to hang down freely into the inner vessel, and is surrounded by a fluid whose composition is the same as that of the

digestive fluid minus the digestive ferment. This parchment dialyser is finally kept in constant up and down motion by means of an appropriate motor, and its contents are thus continuously mixed by the almost "peristaltic" waves which pass over the walls of the tube during its motion. In this way it becomes possible to take advantage of the diffusibility of the digestive products, and effect their more or less rapid removal during any given digestion. It will be further seen that the diffused products can be collected, so that there is no loss of material during the digestion. Moreover, the rate of removal of the products can, within certain limits, be modified by the frequency of changing the fluid into which their diffusive removal takes place. It has not as yet been found possible to supply the third factor in a normal digestion, viz., the continuous addition of fresh digestive ferment. Although the above apparatus is very efficient, as compared with the vessels in which artificial digestions are usually carried on, it falls far behind the natural, and chiefly for two reasons. In the first place, the removal of digestive products is dependent solely and entirely upon their diffusibility, whereas in the alimentary canal there is now no doubt that they are largely removed by the specific activity of the epithelial cells. In the next place, the diffusive exit of the products leads to an influx of fluid into the dialyser, which, by diluting the ferment solution, is detrimental to its continued initial activity. Notwithstanding these shortcomings, the differences in the rate and completeness of a digestion carried on in this apparatus, as compared with those of one carried on under otherwise similar conditions in a flask, are very marked, and indicate that its efficiency is not inconsiderable.

I may now describe the experiments I have made, the results I have obtained, and the conclusions which may be drawn from them.

I.—*The Salivary Digestion of Starch.*

The information we possess indicates that in the alimentary canal starch is completely converted into sugar before absorption. When, on the other hand, starch is digested artificially with either saliva or pancreatic ferment, the conversion into sugar is never anything other than far from complete, and a bye-product, dextrin, is always obtained in varying but large amounts. My first series of experiments was undertaken with a view to determining the cause of the above difference in the two cases, and to obtain, if possible, an artificial digestion which should be as complete as the natural. It may, perhaps, be said that, inasmuch as in the body starch is digested chiefly, and in some animals entirely, by the pancreatic juice, therefore any deduction from a salivary digestion is not applicable to the pancreatic. But this objection is of no great importance here,

for the experiments deal chiefly with the factors which determine the activity of a ferment during digestion, and there is no reason for supposing that there is any fundamental difference in the circumstances which may modify the activities of two ferments so closely similar as those of the salivary gland and pancreas. Indeed, it seems probable that what holds good for saliva will hold good with even greater force for the pancreatic ferment, since the latter is relatively more active than the former.

The experiments were conducted by comparing the products formed when portions of the same mixture of starch and saliva are simultaneously digested for equal times in (i) the dialyser, (ii) a flask. The products with which I had to deal were dextrin and maltose. These I estimated in each case as follows:—The dextrin was precipitated by alcohol in excess, dried at 100°, and weighed; the alcoholic filtrate from this was evaporated to dryness, the residue taken up in water, and the sugar determined in this by rotation and reduction, and no results were accepted in which these two methods did not give equivalent values for the sugar in solution. The following experiments illustrate the accuracy obtainable by these methods. 3·412 grams of starch were placed in each of two flasks and digested with saliva.

Flask 1.—3·412 gr. starch yielded	0·505	gr. dextrin.
	2·838	gr. maltose.
	3·343	
Flask 2.—3·412 gr. starch yielded	0·448	gr. dextrin.
	2·904	gr. maltose.
	3·352	

The results of my numerous experiments are as follows:—

1. The rate of digestion, estimated by the times of relative disappearance of the iodine reactions, is greater in the dialyser than in a flask.

2. The tendency to the development of bacteria is much less in the dialyser than in a flask.

3. The amount of starch finally converted into sugar is always greater than in a flask, and the amount of dextrin residue is less.

This is shown by the following typical experiment.

Using a mixture of starch and saliva which contained 4·23 per cent. of starch, at the end of twenty-one hours' digestion the dialyser contained 16·78 per cent. dextrin, the flask 36·62 per cent. At the end of sixty-eight hours the dialyser contained 8·48 per cent. of dextrin, the flask 35·70 per cent.

4. When the starch is digested in very dilute solutions, the amount changed may be approximately, though never quite, as great in a flask as in the dialyser.

5. The small amount of dextrin (4·29 per cent.) which may be left in the dialyser at the end of an active and prolonged digestion justifies the assumption that under the more favourable conditions in the alimentary canal the whole of the starch would be converted into sugar.

6. There is no evidence from these experiments of the formation in appreciable amount of any sugar other than maltose by the action of saliva on starch.

I am at present engaged on a set of experiments similar to the above, using pancreatic ferment instead of saliva, in order to justify my application of the results obtained with the latter to the natural process, as carried on by the former in the body.

II.—*The Tryptic Digestion of Proteids.*

The starting point for this series of experiments was found in the following considerations. When proteids are digested by trypsin artificially they always yield large quantities of leucin and tyrosin at the same time as the peptones. In the intestine, on the other hand, these crystalline products are not described as occurring in more than microscopic amounts, if at all. Their absence in natural digestion might be due to two causes: either (i) they are formed, but removed as fast as they are formed; or else (ii) they are not formed at all. Since they arise by the further action of the trypsin on the first-formed peptones, and the latter are removed very rapidly after their formation, it was thus possible that the non-occurrence of leucin and tyrosin in the intestine was due to the rapid removal of the material out of which they otherwise would have arisen. It appeared possible to obtain some solution of the difficulties presented by a comparison of natural with artificial digestion by comparative experiments in the dialyser and a flask. The solution is further one of considerable physiological importance, in view of the fact that leucin is known to be partly converted into urea when administered to an animal, so that the difference is one which bears upon the possible source of some of the urea which is normally being excreted under a proteid diet.

The material used for the digestions was fibrin in different conditions: 1. Boiled and extracted with alcohol and ether, in which condition it is extremely difficult of digestion. 2. Simply boiled and dried by pressure. 3. Air-dried without any preliminary boiling. The digestive solution was in most cases prepared by dissolving purified trypsin in 0·25 per cent. carbonate of soda with 0·5 per cent. thymol; in some few cases the fluid was prepared by adding Benger's

"liquor pancreaticus" to the above solution of sodium carbonate and thymol.

The results of the experiments are as follows:—

1. The rate at which the fibrin is broken down is enormously greater in the dialyser than in a flask.

2. The amount of proteid which goes into solution in a given time is always greater in the dialyser than in a flask.

3. The amount of leucin and tyrosin formed in a flask is greater than in the dialyser, but the difference is comparatively slight.

The third of these results made it possible that if, as in the alimentary canal, the peptones could be removed nearly as rapidly as they are formed, then the amount of leucin and tyrosin formed in the dialyser would be also very small, and might approximate to the traces formed in the intestine. This is a possibility to which no experimental test can be applied, since the removal of peptones is in artificial digestions dependent simply on their diffusibility. In the alimentary canal their removal is, without doubt, the result very largely of the specific activity of the epithelial cells. When I examined the contents of the intestine during full proteid digestion, I obtained evidence of a very considerable formation of leucin and tyrosin, of so much, in fact, that the difference with respect to these substances in a natural and artificial digestion becomes quantitative and not qualitative. Thus, in one case where a large dog was fed with 500 grams of lean meat, of which nearly the whole was digested in the ensuing six hours, I obtained such a quantity of leucin and tyrosin from the contents of the small intestine that, after the loss due to recrystallising and the performance of all the tests necessary for their thorough identification, I am in possession of 1 gram of leucin and 0·3 gram of tyrosin. Although the above is the most extreme case of the formation of these substances with which I have met, still in all cases where I have fed dogs with proteid, and this food has been perceptibly digested, I have obtained more than "microscopic" amounts of leucin and tyrosin from the intestine. Since there is no reason to suppose that active absorption of these crystalline products has not been going on during the whole time of their formation, their presence in some cases, in not inconsiderable amounts, towards the close of a digestion must imply a large total formation during the digestion. In this way the difficulty arising out of a quantitative comparison of the crystalline end-products of a natural and artificial tryptic digestion received a solution of a quite unexpected kind.

If, as I believe, my experiments thus show the formation of leucin and tyrosin in not inconsiderable quantities during proteid digestion in the intestine, some interesting considerations arise as to the physiological significance of the same.

Leaving tyrosin out of account for the present, the more usual view has been that any degradation of the nitrogen of proteids into the amide form in the alimentary canal, accompanied by the probable immediate conversion of the leucin into urea in the liver, and its direct excretion as urea, implies a waste of energy which is improbable on teleological grounds. And this view is supported by the statements as to the absence of leucin (and tyrosin) from the alimentary canal. Now, however, we have to deal with the fact that these amidated acids *are* formed during proteid digestion. There are two possible ways in which their formation may be of importance to the animal economy. Although at first sight the conversion of part of the proteid into leucin and its speedy elimination as urea seems to imply a waste of energy to the body, it is not improbable, as Foster has suggested, that this may be of real use to the economy. We know that in artificial digestions the amount, nature, and digestibility of a proteid, and the activity of the ferment which acts upon it, determine to some extent the course and results of the digestion. These factors will probably make themselves still more felt in a natural digestion in the body. Thus leucin may make its appearance to very varying extents at different times in the same animal, and so provide a sort of safety valve to the organism by diverting from the tissues what would otherwise frequently be an unnecessarily large burden of proteid metabolism. A second view is one which opens up the whole question of the physiological significance of the amides in the animal economy. It is *known* that the amides play an all-important part in the nitrogenous metabolism of plants. Leucin and tyrosin occur widely spread in many of the tissues and fluids of animals. In many deranged conditions, notably of the liver, these substances occur in largely increased amounts, so that a discharge of them takes place from the body. It is thus possible that normally their occurrence in smaller amounts is due to the fact that in the healthy organism they are being continually used up in the chemical cycle of tissue metabolism. Thus the real significance of their formation during digestion may well be for the supply of new amidated compounds, to take the place of those portions which must be always becoming useless for further metabolic processes by continual wear and tear. Examined in the light of all the facts which can be brought to bear upon it, there is much evidence in support of this view. It is, however, incompatible with the requirements of a preliminary communication to enter fully here into a discussion and application of this evidence.

II. "On a Fermentation causing the Separation of Cystin." (Preliminary Communication.) By SHERIDAN DELÉPINE, M.B., B.Sc. Communicated by T. LAUDER BRUNTON, M.D., F.R.S. Received February 13, 1890.

During the months of March and April, 1889, I analysed for Dr. Lauder Brunton, and under his direction, a number of specimens of urine containing cystin. The estimation of the amount of this substance present in the samples examined was carried out by Löbisch's process, and revealed certain variations which were of interest as connecting the elimination of cystin with the processes of digestion. In carrying out this work, I was struck with the fact that the amount of cystin precipitated from the same specimen was greater under certain circumstances than under others. Thus, (1) when specimens were *strongly acidified with acetic acid*, as recommended by Löbisch, the precipitation took place more slowly than if the specimens were allowed to undergo a *spontaneous acid fermentation* (which never caused the reaction to become very strongly acid). (2.) When the fluids were *carefully filtered*, the precipitation of cystin was delayed, often for several days. (3.) When a specimen in which cystin had begun to separate was *carefully filtered*, the precipitation was interrupted for several days. (4.) When portions of a urine which was proved by collateral experiments to contain cystin were *kept at a temperature of* 60° C., no cystin could be separated afterwards by the usual processes. (5.) *Evaporation* did not seem to increase materially the amount of cystin obtainable from a given specimen. (6.) The largest amounts of cystin could be obtained by allowing the *specimens to stand at the ordinary temperature for several days*, provided the precipitate was separated whilst the urine was still acid. (7.) A similar amount of cystin could be obtained more rapidly by keeping the fluid at a *temperature of less than* 40° C. for twenty-four to thirty-six hours. (8.) When a drop of urine from which cystin was being deposited, and which contained a large number of bacteria and torulæ, was added to a carefully filtered portion of the same urine, a deposit of cystin occurred in the filtrate thus inoculated in twenty-four hours, the urine becoming at the same time full of bacteria and torulæ, while another portion of the same filtrate not inoculated deposited no cystin for ninety-six hours. I venture to suggest as the most probable explanation of the above results—

(1.) That the *simple* addition of an acid in which cystin is not soluble is not sufficient to separate cystin from the urine, and, therefore, that the *theory generally held as to the state of combination of cystin in the urine is probably inaccurate.*

(2.) That a compound exists in certain urines which under the *influence of a fermentation* yields cystin.

(3.) That the fermentation is due to the *growth of an organism*, which can apparently be separated from the urine by ordinary filtration, and must therefore be a *large organism, possibly a torula.*

(4.) That the cases recorded in which cystin has been found deposited in the kidneys and liver indicate that the *fermentation may begin in the system.*

III. "Some Stages in the Development of the Brain of *Clupea harengus.*" By Ernest W. L. Holt, Marine Laboratory, St. Andrews. Communicated by Professor McIntosh, F.R.S. Received February 11, 1890.

(Abstract.)

The stages described are (i) newly-hatched or early larval; (ii) early post-larval; (iii) $\frac{1}{2}$-inch long; (iv) $\frac{3}{4}$-inch long.

The development of the pineal region is treated separately, and in this a fifth stage—$1\frac{1}{12}$-inch long—is introduced.

In the early larval stage the downward flexure of the fore part of the brain is very noticeable. It appears due to the general conformation of head at this stage. The cerebral lobes are short; the anterior commissure is well marked. The white matter of the cerebrum is divided into two patches on each side, from the most ventral of which the short stout olfactory nerves pass to the bases of the nasal sacs, now closely opposed to the cerebrum. The roof of the cerebrum is very thin, passing into the thicker roof of the thalamencephalon. The tips of the optic thalami are wholly vesicular. A diverticulum of the 3rd ventricle extends downwards and backwards, its distal extremity underlying the optic commissure. The broad ventral commissure of the infundibulum, noticed in *Anarrhicas*,* is well marked. A commissure shuts off the lumen of the infundibulum from the hind part of the 3rd ventricle immediately in front of the splitting off of the infundibulum. The optic ventricles do not appear in the front part of the mid-brain, and are only partially developed further back. The tori semicirculares are present in the hind part of the mid-brain as mounds on either side of the central fissure of the cerebral mass. The valvula appears in transverse section as a pair of ridges externally to the tori, before it shuts off the aqueduct of Sylvius. The cerebellar fold is very short; the pituitary body is a roundish mass of deeply staining cells, opposed ventrally to the membranous roof of the mouth, and clasped in front and at the sides

* McIntosh and Prince, 'Edinb. Roy. Soc. Trans.,' vol. 35.

by the walls of the infundibulum. These break down above the body, except for a fine cellular membrane. Behind the body is seen the tapering, downwardly-bent anterior end of the notochord.

In the early post-larval stage* "an apparent rectification of the cranial axis" has taken place, by the upward rotation of the cerebrum on its posterior end, doubtless owing to the rapid development of the oral and trabecular cartilages, and consequent forward rotation of the mouth. The same causes have also operated so as to withdraw the diverticulum of the 3rd ventricle from its position below the optic commissure. Changes are noticed in the arrangement of the nervous tissues. The olfactory nerves are longer, and the nasal sacs further from the brain. The fibrous bridge over the 3rd ventricle (behind the pineal body), terminating with the posterior commissure, is well marked. The tips of the tectum lobi optici are seen above it. The bases of the optic thalami (walls of the thalamencephalon) have increased in breadth. The mid-brain is comparatively large, and the optic ventricles much more advanced. The cerebellar fold rapidly thins out in the middle, assuming the form, in transverse section, of thick lateral elements united by a cellular band. The infundibulum has undergone vertical flattening. The future lobi inferiores are indicated as lateral expansions, behind which the 3rd oculomotor nerves pass outwards from the centre of the ventral surface of the cerebral mass. The infundibulum extends some way back above the notochord as a thin-walled sac. Its walls are little plicated compared with those in some other forms, *e.g.*, *Rhombus*,† *Anarrhicas*.‡

In the ½-inch stage the olfactory lobes appear as bulbous masses projecting from the front end of the cerebrum. Fibres can be traced from the optic nerves up to the fore part of the optic lobes. A pale median septum appears between the anterior extremities of the lateral optic ventricles, its base resting on the fibrous tract over the hind part of the 3rd ventricle. The tip of the valvula now appears in transverse section before its connexion with the cerebral mass can be made out, having thus grown forward. The cerebellum has greatly increased in size: instead of terminating as before on the surface of the brain, it is now continued into a thick fold bent sharply down on the anterior portion; its posterior end passes at once into the thin roof of the 4th ventricle. Two fibrous bands cross over the aqueduct of Sylvius in the substance of the cerebellum; their lateral extremities are fused. The lobi inferiores are better marked than in earlier stages. Longitudinal bands of fibres pass back from the roots of the oculomotor nerves through the medulla oblongata. Groups of large ganglionic cells appear on either side of these bands,

* Balfour, 'Development of Elasmobranch Fishes.'

† Stieda, 'Zeitschr. Wiss. Zool.,' 1869.

‡ McIntosh and Prince, *op. cit.*

and are connected by a fine commissure passing through both bands. At the origin of the VIII auditory nerves, this commissure is replaced by a St. Andrew's cross of fibres, the dorsal limbs of the cross passing to the nerve roots, and the ventral to the ganglionic areas.

In the $\frac{3}{4}$-inch stage the olfactory lobes are more elongated. The olfactory nerves pass outwards from their anterior extremities. The septum behind the pineal body is larger, and contains a few very large cells. The septum, after losing its ventral connexion with the fibrous tract over the 3rd ventricle, persists for some way back as a cellular leaf-like appendage of the thin median roof of the optic ventricle; a few fibres pass back into this appendage.

The optic nerves are longer, from the outward displacement of the eyes. They are very stout and solid, and from their roots fibres may be very easily traced into the optic lobes. Fibres are seen passing from the cerebral mass across the optic ventricle, external to the tori, to the tectum lobi optici.

The flattening of the brain, noticed by McIntosh and Prince in the herring of $\frac{7}{24}$ inch, is intensified at this stage, and the brain is also much elongated.

Large ganglionic cells appear in the tori semicirculares about the region of the splitting off of the infundibulum. The white matter of the tectum lobi optici is very conspicuous, and shows traces of three cellular layers in its substance. A circular pale area appears amongst the vesicular matter on either side of the valvula. The lobi posteriores are present. Behind them the walls of the medulla approach each other dorsally, shutting off the central canal from the 4th ventricle. The walls recede again further back before finally closing opposite the last trace of the auditory capsule.

From behind the region of the auditory nerves a ganglionic area on either side persists backwards through the medulla oblongata. The cerebellum has increased in bulk; its anterior dorsal angle is carried forward. The fibrous bands previously noticed are carried further back, and now lie clear of the optic lobes. Three smaller fibrous bands occur behind them. In the herring of $1\frac{1}{12}$ inch the two first fibrous bands are fused together.

Pineal Region.

The roof of the thalamencephalon in the early stages is a single layer of large columnar cells passing forward from the front wall of the pineal stalk. It passes into the roof of the cerebrum, the cells diminishing greatly in size. The superior commissure of Osborn is present from the early post-larval stage; it is also present in the larval and post-larval *Zoarces viviparus*, where it is distinctly double. The first signs of the infrapineal recess of Hoffmann are seen in the

$\frac{1}{2}$-inch stage. It is thus much later in developing than in *Salmo;** and the fold forming its front wall never extends backwards to the same degree as in that form and in *Anarrhicas.* This fold, in the post-larval *Zoarces*, is thickened in its apex, and lodges a fine commissure. As pointed out by Balfour in Elasmobranchs the fold is due to the upward rotation of the cerebrum.

The fibrous tract over the 3rd ventricle in the herring is well marked in the $\frac{3}{4}$-inch stage. It is seen to consist of fibres passing upwards and inwards from the optic thalami to the middle line above the 3rd ventricle, and then running forward to the stalk of the pineal body. The tract has a double nature, as is readily seen in vertical longitudinal sections of a herring $1\frac{1}{12}$ inch long. It is seen here to be a backwardly directed fold of the brain roof, continuous ventrally with the back wall of the pineal stalk, and dorsally with the roof of the optic ventricle, the apex of the fold being the posterior commissure. Its length in this form is due to the flattening of the brain, the tract being very short in *Zoarces*, where the brain is not flattened. In *Zoarces*, also, from the same cause, the limbs of the fold are less closely applied to each other and much thicker.

The pineal body is roundish and solid in the early larval stage in the herring. It is vertically flattened in the early post-larval stage. In the $\frac{1}{2}$-inch stage it is much larger and contains a lumen; it shows signs of constriction into proximal and distal elements, and the lumen contains a coagulable albuminous fluid, as in *Petromyzon.*† In the $1\frac{1}{12}$-inch stage the constriction is still visible, and the walls are generally crenated. The tissues of the pineal wall are now divided into three layers, and are of varying thickness. The cartilage of the tegumen cranii overlies the body at this stage. The constriction of the body appears to be an exaggeration of the crenation of the pineal wall met with in *Salmo;* it has not, probably, the morphological value of the constriction of the body in *Petromyzon.*

IV. "A Cyanogen Reaction of Proteids." By J. GNEZDA, M.D. Communicated by Professor E. A. SCHÄFER, F.R.S. (From the Physiological Laboratory, University College, London.) Received December 19, 1889.

When dry urea is heated to its melting point it gives off ammonia, and a substance called biuret ($C_2N_3H_5O_2$) remains behind. Biuret is decomposed by heat into ammonia and cyanuric acid ($C_3N_3H_3O_3$).

* Hoffmann, "Zur Ontogenie der Knochenfische," 'Arch. Mikrosk. Anat.,' vol. 23, 1884.

† Beard, "Parietal Eye in Cyclostomatous Fishes," 'Quart. Journ. Micros. Sci.,' 1889.

G. Wiedemann* discovered that on adding an alkaline solution of copper sulphate to cyanuric acid, a violet solution was produced. The same investigation showed that biuret gave a rose-red solution on treatment with copper sulphate and sodium hydrate.

One of the ordinary tests for a proteid, which is sometimes called Piotrowski's reaction, is the violet solution produced by adding copper sulphate and caustic potash or soda. Albumoses and peptones differ from other proteids in giving with these reagents a rose-red instead of a violet solution; the colour so produced is the same as that given by biuret, hence this reaction is generally spoken of as the *biuret reaction.* It is stated that after heating any proteid with caustic soda, and then adding copper sulphate, the rose-red coloration of the biuret reaction appears; and this is undoubtedly the case, for the action of the hot concentrated alkali is to form not only alkali-albumin, but also some substances of the albumose class. The whole value of the test in distinguishing between native albumins on the one hand, and peptones and albumoses on the other, depends on its being performed in the cold.

Brücke† especially has emphasised the difference between the violet coloration given by ordinary proteids and the rose-red (so-called biuret) reaction of the peptones. He however considers that the radicle in the complex proteid molecule which gives rise to the reaction is probably the same in both cases, but that it is some other change in the molecule that causes the difference of tint.

Salkowski‡ has recently investigated the colour reactions of the proteids, and shown that Millon's reaction and the Adamkiewicz reaction are produced by the presence in the proteid molecule of certain aromatic radicles. He does not, however, appear to have investigated the biuret reaction; and the object of my own work has been to discover, if possible, upon what groups of atoms in the proteid molecule this reaction depends. Whether the violet colour of ordinary proteids is due to cyanuric acid, and the rose-red colour of peptones to biuret, I am, however, unable to say. The main result of my experiments has been that these reactions depend on the presence in a proteid of cyanogen, or of some cyanogen-containing radicle. I have also, by means of somewhat modifying the method usually adopted for performing the test, discovered that it may be used for distinguishing classes of proteids from one another more accurately than has hitherto been possible.

The chief modification I have introduced into the test has been the addition of ammonia, either instead of or in addition to the potash or

* 'Poggendorff's Annalen,' vol. 74, 1849, p. 67.

† *Sitzungsberichte* of the Vienna Academy, 1883, reprinted in 'Monatshefte f. Chemie,' vol. 4.

‡ 'Zeitsch. f. Physiol. Chem.,' vol. 12.

soda usually employed. In some cases I have added each reagent separately, first copper sulphate, then ammonia, and then potash or soda; but, as a rule, I have employed a reagent made by dissolving a copper salt in ammonia; the dark blue solution that results is an ammoniacal solution of cupric hydroxide. After adding this to a solution of proteid and observing the result, potash or soda can be added subsequently. The use of the ammoniacal solution of cupric hydroxide as a reagent for detecting proteids was first suggested to me by the fact that on adding some of it to urine from a case of cystitis I obtained a reddish-violet colour. In the light of subsequent experiments, it is probable that this urine contained a peptone or peptone-like substance derived from the decomposition of pus corpuscles.

The reactions I obtained with various proteids may be thus tabulated:—

Proteids.	Addition of ammoniacal solution of cupric hydroxide produced:—	Subsequent addition of potassium or sodium hydroxide produced:—
Egg albumin.	Pale blue solution.	Violet solution.
Serum albumin.	Pale blue solution.	Violet solution.
Grübler's peptone.	Violet solution.	Rose-red solution.
Witte's peptone.	Violet solution.	Rose-red solution.
Pure peptone (prepared from Witte's peptone).	Violet solution.	Rose-red solution.
Albumoses (prepared from Witte's peptone).	Violet solution.	Rose-red solution.

Native albumins differ from the products of proteolysis (albumoses and peptones) by giving no change of colour with an ammoniacal solution of cupric hydroxide; when potash or soda is subsequently added, the result is, as usual, a violet solution. The albumoses and peptones, on the other hand, give a violet solution with the ammoniacal cupric hydroxide; this is turned red on the subsequent addition of potash or soda.

Copper sulphate and ammonia added separately give the same results. When a drop of copper sulphate solution is added to a proteid solution, the result is a precipitate of an albuminate of copper;* on adding ammonia, this dissolves up; if the solution is blue, changing to violet when potash is added, albumoses and peptones are absent; but if the solution is violet, changing to red when potash is added, albumoses or peptones, or both, are present.

* This preliminary precipitation does not, however, occur with deutero-albumose nor with pure peptone.

This method of performing the test has a great advantage over the way in which it is usually done, as it is much easier to distinguish between the blue of the dissolved cupric hydroxide and the violet due to peptone when ammonia is added than it is between the violet solution given by albumin and the rose-red given by peptones when potash is added, especially if the solutions be dilute. The test with ammonia has also this advantage, that peptones and albumoses can be detected with certainty even if other proteids are present at the same time.

I next proceeded to make experiments with nickel sulphate dissolved in ammonia; the solution so formed is a purplish one, and the results obtained may be tabulated as before:—

Proteid.	Addition of nickel oxide in ammonia produced:—	Subsequent addition of potassium or sodium hydroxide produced:—
Egg albumin.	Faint bluish solution.	Yellow solution.
Serum albumin.	Faint bluish solution.	Yellow solution (with flocculent precipitate).
Witte's peptone.	Yellow solution (with flocculent precipitate).	Orange solution (with flocculent precipitate).
Grübler's peptone.	Ditto.	Ditto.
Pure peptone.	Ditto.	Ditto.
Albumoses.	Ditto.	Ditto.

This test may thus be used for distinguishing between albumins and the products of proteolytic digestion; the former giving a yellow solution only after the addition of potash or soda, the latter giving a yellow colour with nickel oxide and ammonia alone, which is however deepened to a dull orange by the addition of potash or soda.

I next proceeded to apply these tests to other classes of proteids, albuminates, globulins, fibrin, coagulated proteid, and mucin; and the results are stated in the following table:—

Proteid.	Addition of cupric hydroxide in ammonia produced :—	Potash or soda then added produced :—	Nickel oxide in ammonia produced :—	Potash or soda then added produced :—
*Albuminates.**				
Acid albumin.	Blue solution.	Violet solution.	Pale blue solution.	Yellow solution.
Alkali albumin.	Blue solution.	Violet solution.	Pale blue solution.	Yellow solution.
Casein.	Blue solution.	Violet solution.	Pale blue solution.	Yellow solution.
Globulins.†				
Serum globulin.	Blue solution.	Violet solution.	Pale blue solution.	Yellow solution.
Vitellin.	Blue solution.	Violet solution.	Pale blue solution.	Yellow solution.
Myosin.	Blue solution.	Violet solution.	Pale blue solution.	Yellow solution.
Fibrin.	Blue.	Violet.	Pale blue.	Yellow.
Coagulated proteid.	Violet.	Rose-red.	Yellow.	Orange.
Mucin.	Blue solution.	Violet solution.	Pale blue solution.	Yellow solution.

* The acid albumin and alkali albumin were prepared from egg albumin by adding to it a few drops of very dilute acid and alkali respectively, and warming to 40° C. for 10—15 minutes.

† Dissolved in 1 per cent. ammonium sulphate solution or a dilute magnesium sulphate solution.

Coagulated proteid behaves like the peptones; this is easily explicable, as Neumeister* has shown that the action of hot water on proteids forms from them small quantities of albumoses due to hydration. The other proteids in the above list behave like the albumins, and thus differ from the albumoses and peptones.

I now pass from the proteids to certain derivatives of proteids, these experiments being designed to elucidate the question as to what radicle it is on the proteid molecule to which these reactions are due.

Uric Acid—Uric acid was dissolved in soda and boiled; then cooled, and a drop of copper sulphate added; this did not colour the fluid at all; on adding more, a violet solution was obtained.†

Uric acid dissolved in soda, but not heated, gave with cupric hydroxide in ammonia a pink colour.

A little uric acid was evaporated to dryness on a porcelain dish with nitric acid; on adding cupric hydroxide in ammonia to this a violet colour is obtained; this is not simply the murexide test produced by the ammonia, as nickel oxide dissolved in ammonia gives a deep yellow colour. Thus uric acid gives the reactions very much as proteids do.

Xanthine, *hypoxanthine*, and *sarcosine* give the same reactions.

Biuret and *cyanuric acid* are the substances in which the colour tests with cupric hydroxide in alkaline solutions were first observed; the following are the particulars of the experiments I have performed with these two substances:—

Addition of—	To aqueous solution of biuret.	To aqueous solution of cyanuric acid.
(1.) Cupric sulphate.	No effect.	No effect.
(2.) Cupric sulphate and potash.	Rose-red solution.	Violet solution.
(3.) Cupric sulphate and ammonia.	Blue solution.	Blue solution.
(4.) Cupric sulphate and ammonia, followed by potash.	Rose-red solution.	Violet solution.
(5.) Nickel sulphate dissolved in ammonia.	Blue solution.	Blue solution.
(6.) Nickel sulphate in ammonia, followed by potash.	Orange solution (with flocculent precipitate).	Yellow solution (with flocculent precipitate)

* 'Zeitsch. f. Biol.,' vol. 24, p. 272.

† Winogradoff ('Virchow's Archiv,' vol. 27, p. 565) and Worm-Müller ('Pflüger's Archiv,' vol. 27, p. 31) mention something similar, but not in connexion with the present subject.

Biuret thus behaves in very much, but not exactly, the same way as peptones; while cyanuric acid gives the same colour reactions as albumin.

Hydrocyanic Acid.—The same series of reactions was tried with this substance, and the result was that the colours obtained were precisely the same as those obtained with peptones and albumoses. The details are as follows:—

Addition of	Produced
Cupric sulphate	No effect.
Cupric sulphate and potash	Rose-red solution.
Cupric sulphate and ammonia, or ammoniacal solution of cupric hydroxide	Violet solution.
Subsequent addition of potash or soda	Rose-red solution.
Nickel sulphate dissolved in ammonia	Yellow solution.
Subsequent addition of potash or soda	Orange solution.

The colours produced are in some cases evanescent, and, if any free acid is left not neutralised by the ammonia or potash added, the liquid remains colourless.

Glycocine, Leucine, Tyrosine.—These substances gave negative results, the liquid remaining blue or bluish-green throughout.

Ethyl aldehyde, propyl aldehyde, valeraldehyde, isobutyl aldehyde, and *benzyl aldehyde* similarly gave entirely negative results.

General Conclusions.

The addition of cupric sulphate and potash to albumin or globulin produces a violet solution. The addition of the same reagents to peptones or albumoses causes a rose-red solution. If ammonia be added as well the results are as follows:—Cupric sulphate and ammonia added to albumin causes a blue solution, turned violet on the addition of potash; cupric sulphate and ammonia added to peptone or albumose gives a violet solution, turned red on the addition of potash.

By this reaction, and by a somewhat similar reaction in which nickel sulphate is used, peptones and albumoses may be easily distinguished from, and detected in the presence of, albumins and globulins.

Not only proteids, but other organic substances ultimately obtainable from proteids, give very similar reactions.

The reaction is not given by the amido-acids, glycocine, and leucine; nor by derivatives, like tyrosine and benzyl aldehyde, that contain an aromatic nucleus, nor by the aldehydes of various alcohols. But in the substances that do give the test, the nitrogen is either partly or wholly combined in the form of cyanogen: these substances are biuret, cyanuric acid, uric acid, xanthine, hypoxanthine, sarco-

Reagent added.	Colour of resulting solutions with				
	Albumins and globulins.	Peptones and albumoses.	Biuret.	Cyanuric acid.	Hydrocyanic acid.
Copper sulphate + ammonia	Blue.	Violet.	Blue.	Blue.	Violet.
Copper sulphate + potash	Violet.	Rose-red.	Rose-red.	Violet.	Rose-red.
Copper sulphate + ammonia + potash......	Violet.	Rose-red.	Rose-red.	Violet.	Rose-red.
Cupric hydroxide dissolved in ammonia......	Blue.	Violet.	Blue.	Blue.	Violet.
Cupric hydroxide dissolved in ammonia + potash	Violet.	Rose-red.	Rose-red.	Violet.	Rose-red.
Nickel sulphate dissolved in ammonia	Blue.	Yellow.	Blue.	Blue.	Yellow.
Nickel sulphate dissolved in ammonia + potash	Yellow.	Orange.	Orange.	Yellow.	Orange.

sine, and hydrocyanic acid. It thus appears probable that the colour reaction of the proteids that occurs on addition of a cupric salt and an alkali is due to the existence in the proteid also of cyanogen.

Just as some proteids give a rose-red colour, and others a violet, so, in the list of substances just enumerated, some give a rose-red, and some a violet. Biuret was the substance in which a rose-red colour was first noted; hence the term biuret reaction, as applied to peptones. Cyanuric acid was the substance in which a violet colour was first noted. Probably in both cases the reaction is due to a cyanogen radicle; but the cause of the difference in colour is unknown. In the same way, our ignorance of the constitution of the proteid molecule stands in the way of our discovering the difference between peptones that give a rose-red colour and albumins that give a violet colour.

The term biuret reaction is to some extent a misnomer, as applied to the peptones and albumoses; the test with the modification I have introduced behaves a little differently in the two cases. The substance that peptone most nearly resembles in its colour reactions is hydrocyanic acid, as is shown in the table (p. 209), in which a contrast is drawn between the chief substances which I have examined.

Using the word cyanogen in the widest possible sense, the conclusion I should draw from such a series of experiments is that the colour reaction with a cupric salt and an alkali is a cyanogen reaction. Among the simpler organic bodies examined, we have certain cyanogen compounds, like cyanuric acid, that give a violet colour; and certain proteids (the albumins and globulins) give the same colour. There are certain other substances, like biuret, which give a red colour without any intermediate violet stage; there are others, like hydrocyanic acid, which give a violet colour with ammonia, which is turned red by potash or soda; and to this last group the peptones also belong. Just as there is a different combination of the cyanogen in cyanuric acid from that in hydrocyanic acid, so there is probably the same difference between the combination of the cyanogen in albumin and peptone respectively; and this difference is, as a rule, brought about by a digestive ferment.

Presents, February 20, 1890.

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February 27, 1890.

Sir G. GABRIEL STOKES, Bart., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The Croonian Lecture was delivered as follows:—

CROONIAN LECTURE.—"The Relations between Host and Parasite in certain Epidemic Diseases of Plants." By H. MARSHALL WARD, M.A., F.R.S., late Fellow of Christ's College, Cambridge, Professor of Botany in the Forestry School, Royal Indian Engineering College, Cooper's Hill. Received February 27, 1890.

(Abstract.)

Pointing out the intimate relations between the study of plant physiology and pathology, the lecturer briefly referred to the existing modes of classifying the diseases of plants, and the difficulties they involve. Broadly speaking, there are diseases due to soil, climate, and the influence of the non-living environment on the one hand; and those due to the attacks of living organisms (parasitic fungi, insects, &c.) on the other. Some interesting cases were briefly discussed, and the fact brought out that several causal factors co-operate in producing any disease.

With regard to fungus diseases, there are especial complexities, because we have to learn (1) the life history of the fungus, and (2) understand the biology of the host-plant, and this means we must (3) also discover what influences in each case are exerted by the variations of the environment (heat, light, moisture, &c.) in each case. Even then there is an unknown variable (4) in the internal changes going on in both the host and the parasite.

After reviewing, with the aid of illustrations and experiments, some of the principal functions of the normal tissues of a green plant, the effects of variations in temperature, intensity of light, amount of water in the air, &c., were discussed. The chief points are that under certain conditions, *e.g.*, a low temperature, feeble light, and when the atmosphere is saturated with moisture, the plant may be less able to withstand the inroads of a parasite, because its protective cell-walls are thinner and more watery, its cell sap abounds in sub-

stances like glucose, acids, and soluble nitrogenous matters, the protoplasm lining each cell is less capable of destroying substances which can injure it—its respiratory processes being enfeebled—and, in short, such a plant approaches the condition of a very young seedling, or a plant growing in the dark.

Experiments have proved that such plants not only offer less resistance to the hypha of a parasite, but the very conditions which cause the plant to abound in materials suitable to the fungus also suit the fungus itself.

Attention was then called to a peculiar parasitic disease, very common in green-houses, gardens, &c., in this country and elsewhere, and some cuttings of geraniums which had been wholly or partially destroyed by it were exhibited. One curious fact is, that this fungus causes a sort of rotten-ripeness of grapes on the Rhine, and that these mouldy grapes are those which are used to produce the finest wines in some of the districts: the explanation is that the diseased grapes undergo remarkable changes, by which the proportion of acid is reduced, and the must of the grapes rendered richer. But although in this case we utilise the effects of the disease-producing fungus, in other cases these fungi cause epidemic diseases of clover, rape, hemp, onions, hyacinths, and other plants.

The symptoms and progress of these diseases were described, the chief points being illustrated by lantern slides and actual specimens, of which there was a collection on the tables.

The fungus attacks the plant by destroying first its cell-walls, and then its protoplasm, cell by cell: this it effects by excreting a series of ferments or poisons. When it has destroyed the tissues, the fungus proceeds to extend more rapidly, and the destruction quickly advances. The fungus is as it were in the position of an attacking army, its weapons being these soluble ferments, or poisons, capable of dissolving the cell-walls and killing the living protoplasm in the cells.

The tissues of the host-plant, again, are in the position of a besieged army; the real fighting force being the protoplasm. The protoplasm is entrenched, so to speak, behind the cellulose cell-walls, and it has in its interior stores or reserves of food-materials which may be in a well replenished condition or the reverse. The hyphæ of the fungus overcome the cell-walls or out-works, by dissolving them by means of soluble ferments, and it will be intelligible that the thickness and solidity of these cell-walls are important in the matter; thin, soft, watery cell-walls being more easily penetrated.

Once inside the walls, the fungus-hypha is face to face with the real fighting contingent, however, the protoplasm; and the fact was insisted upon that circumstances affect the power of this protoplasm to cope with the poison which the hyphæ pour into it. So long as the

protoplasm can dispose of the small amounts of poison coming in from the hyphæ, by respiratory oxidation or otherwise, the hypha is debarred access to the cells, but immediately the poison succeeds in lessening or destroying the power of the protoplasm to control the cell-sap, the latter exudes through the permeable protoplasm, and suffuses the whole tissues with acid sap, containing just such food-materials as the fungus flourishes in. Consequently the latter spreads quickly, killing the cells more rapidly than ever, and soon destroying large tracts of tissue. This killed tissue turns brown, and we can consequently trace the progress of the disease by the spread of the discoloration. It was shown that the destructive power of the fungus concerned—*i.e.*, the capability of its hyphæ to produce the poisons—can be enhanced by culture in solutions of sugar, organic acids, and a little nitrogenous material and salts—just such a solution as is obtained in infusions of dead vegetable tissues; consequently the destructive power of the parasite increases as it feeds on the proceeds of destruction.

It has recently been discovered that the successive crops of spores of the fungus differ in infective power, and that, whereas the spores first formed may be unable to infect a living plant, those of the second or third generation can do so.

In conclusion, and passing over the observations and references to other diseases, there are four chief points to be considered in regard to the epidemic fungus-diseases concerned.

First, there is the healthy host-plant itself, which may be a more or less favourable object for the fungus. Secondly, there is the fungus, which may, or may not be, able to kill the living cells of the host. Thirdly, the influence of variations in the environment—especially low temperatures, want of light, and damp air—may so affect the host-plant that it is more easily and quickly infected by the fungus than was the case when its cell-walls were thicker and harder, and its protoplasm more capable of effecting certain processes of metabolism, and controlling the sap in the cells. Fourthly, the fungus also is capable of being rendered more formidable by variations in its environment, and especially by invigorating culture in suitable food materials.

Now when the external conditions are such that they favour the development of the fungus, while at the same time lowering the metabolic activity and respiratory power of the protoplasm, the conditions for an epidemic of the disease in question exist, and this is frequently realised in a dull, cold, wet July or August in this country. The point especially insisted on is not that any mysterious predisposition to disease is here manifest, but that the one plant—the fungus—is favoured by the prevailing conditions of culture more than the other—the host-plant. If we wanted to culti-

vate the fungus in one green-house and the host in another, each by itself, we should endeavour to provide the one set of conditions for the fungus, and another and very different set for the host.

Presents, February 27, 1890.

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"On the Steam Calorimeter." By J. Joly, M.A., B.E., Assistant to the Professor of Civil Engineering, Trinity College, Dublin. Communicated by Professor Fitzgerald, M.A., F.R.S., F.T.C.D. Received November 26,—Read December 19, 1889.

[Plates 6, 7.]

In two papers read before the Royal Society,* some three years ago, I described a "Method of Condensation" in calorimetry. A number of experiments in support of the reliability of the new method are contained in those papers, as well as a description of such forms of apparatus as I had then been using. The apparatus, however, could not be said to be the result of a very prolonged study of the capabilities of the method, and possessed many defects, chiefly on the score of convenience in effecting the measurements. A continued use of the method since that time and its application to some exacting measurements have led to various alterations in the apparatus, so that, after many reconstructions, entirely new forms have been conferred on the instrument. I purpose to describe two new forms: a single calorimeter similar in type to the older instrument, but differing in construction; and a differential calorimeter, rendering possible measurements which could hardly be effected in the single type of instrument.

In the interval, too, a wider knowledge of the capabilities of the method has been acquired. Its errors have been enquired into. On the question of the errors arising from radiation many hundreds of experiments have been made. The general results of these will be found in the following pages. Again, I have from time to time tabulated such data as are of use in the applications of this calorimetrical method. These I ask permission to include, so as to render this account of the method as complete as it can, within convenient bounds, be made. As, however, descriptions of the principles of the method, and of many experimental tests to which it has been subjected, are accessible both in Professor Bunsen's paper† on the

* "On the Method of Condensation in Calorimetry" and "On the Specific Heats of Minerals," 'Roy. Soc. Proc.,' vol. 41, p. 352 *et seq.*

† "Über das Dampfcalorimeter," Wiedemann's 'Annalen der Physik und Chemie,' vol. 31, 1887, p. 1.

"Steam Calorimeter" (as he has designated it) and in my own papers (*loc. cit.*), I will go over this old ground only so far as to be intelligible to those who have not seen those papers.

Theory of the Method.—The theory of the method is, briefly, as follows:—A substance at the temperature, t_1, of the air brought into an atmosphere of saturated steam will, in attaining the temperature, t_2, of the latter, condense a certain weight of steam, w, such that $w\lambda$, where λ is the latent heat of vapour of water, represents a quantity of heat equal to the calorific capacity of the substance between the limits of temperature. Hence, if S be the specific heat of the body, W its weight,

$$w\lambda = \mathrm{WS}\ (t_2^\circ - t_1^\circ) \dots\dots\dots\dots\dots \quad (1.)$$

From this S is deduced by measuring w, t_2°, t_1°, W, and knowing the value of λ from recorded experiments.

The apparatus required is one permitting the sudden admission of steam around the substance, and subsequently the accurate observation of the weight of water precipitated upon it.

The steam calorimeter is on the lines of a slight metal receptacle, placed beneath a delicate balance, so that a wire depending from one arm of the balance sustains a light wire platform within the receptacle or calorimeter. The platform is provided with a little platinum-foil catchwater beneath it. The substance to be dealt with is placed upon the platform. Steam being admitted into the calorimeter, the substance rapidly rises to its temperature, condensing steam, which adhering as water to its surface, or dropping into the catchwater beneath, is estimated without loss by the balance. In this way the value of w is determined in the equation for the specific heat.

The observation of t_1° is effected by a thermometer left in company with the substance in the calorimeter a sufficient length of time and read just before admitting steam. The temperature of the steam, t_2°, is deduced by inserting a thermometer in the calorimeter when it is filled with steam or by observation of the height of the barometer. In the thermometry it is sufficient in order to secure a high degree of accuracy to read the second place of decimals by estimation, the thermometers having a fairly open scale divided to tenths of degrees. The range obtaining is so considerable that one tenth of a degree is a small fraction of the whole.

On the Values of the Constants required and the Corrections necessary in the Use of the Calorimeter.

The succeeding pages contain a discussion of the constants required in the use of the steam calorimeter and the mode of applying the necessary corrections.

The Latent Heat of Steam.—The value of λ, the latent heat of steam, may be taken for rough experiments as 536·5, its value at 760 mm. pressure. In accurate work its variability with the barometric height is, however, not negligible. Following Professor Bunsen, and in a great measure quoting from his paper, I subjoin a table containing the value of λ and the boiling point of water at different heights of the barometer. The values of λ are calculated on Regnault's formula for the total heat of steam :—

$$Q = 606{\cdot}5 - 0{\cdot}695t - 0{\cdot}00002t^2 - 0{\cdot}0000003t^3.$$

Table I.—Pressure, Temperature, and Latent Heat of Steam.

Pressure in mm.	Temp. of steam.	Latent heat.	Pressure in mm.	Temp. of steam.	Latent heat.
726	98·72	537·4	759	99·96	536·5
727	98·76	537·4	760	100·00	536·5
728	98·80	537·3	761	100·04	536·5
729	98·84	537·3	762	100·07	536·5
730	98·88	537·3	763	100·11	536·4
731	98·92	537·2	764	100·15	536·4
732	98·95	537·2	765	100·18	536·4
733	98·99	537·2	766	100·22	536·4
734	99·03	537·2	767	100·25	536·3
735	99·07	537·1	768	100·29	536·3
736	99·11	537·1	769	100·33	536·3
737	99·14	537·1	770	100·36	536·3
738	99·18	537·1	771	100·40	536·2
739	99·22	537·0	772	100·44	536·2
740	99·26	537·0	773	100·47	536·2
741	99·29	537·0	774	100·51	536·2
742	99·33	537·0	775	100·54	536·1
743	99·37	536·9	776	100·58	536·1
744	99·41	536·9	777	100·62	536·1
745	99·44	536·9	778	100·65	536·1
746	99·48	536·9	779	100·69	536·0
747	99·52	536·8	780	100·72	536·0
748	99·56	536·8	781	100·76	536·0
749	99·59	536·8	782	100·80	536·0
750	99·63	536·8	783	100·83	535·9
751	99·67	536·7	784	100·87	535·9
752	99·70	536·7	785	100·90	535·9
753	99·74	536·7	786	100·94	535·9
754	99·78	536·7	787	100·98	535·8
755	99·82	536·6	788	101·01	535·8
756	99·85	536·6	789	101·05	535·8
757	99·89	536·6	790	101·08	535·8
758	99·93	536·5			

The Density of Steam and Correction for Displacement.—Allowance must further be made for the change of density of the medium surrounding the substance. This will in many cases amount to a

considerable deduction from the increase of weight indicated by the balance. It is to be remembered that the density of steam at 100°C. is about half that of air at 10°C. The effect on the apparent weight of the substance will in fact be observable even if it displace a volume of but one cubic centimetre, and the deduction becomes very necessary when dealing with bulky substances.

The following table contains the density of saturated steam over the range of barometric variation. It is calculated from the formula of Zeuner,*

$$y = \alpha p^{1/n},$$

in which, when p is expressed in atmospheres, a has the value 0·6061, $1/n$ the value 0·9393.

Table II.—Mass of a Cubic Centimetre of Saturated Steam in Grams.

Pressure in mm.	Grams.	Pressure in mm.	Grams.	Pressure in mm.	Grams.
730	·000583	750	·000598	770	·000613
735	·000587	755	·000602	775	·000617
740	·000591	760	·000606	780	·000621
745	·000594	765	·000610	785	·000625

The results obained from this formula and embodied in the table agree well with deductions based on Regnault's experiments on the total heat of steam. The rate of variation with rise of temperature is closely represented by the formula, but it is noteworthy that the values themselves depart somewhat from Fairbairn's and Tate's experimentally found values.† Thus, according to the latter observers, the density at 100° is 0·0006187. Now, although in general a small error in those values is not of great import—an error of as much as 5 per cent. would most generally have an inappreciable effect on the estimation of w—yet cases may arise when a close value is desirable.

In the hopes of deciding between the various values assigned to the density of steam, I made some direct experiments in the calorimeter. These, in fact, became necessary in the course of some early experiments on the specific heat of air at constant volume, when the displacement difference of a spherical copper vessel having a volume of about 164 c.c. had to be considered. Although the experiments are not as concordant as could be desired, their object is, I think, sufficiently attained. I, therefore, add a short account of them here.

* 'Théorie Mécanique de la Chaleur,' p. 286.

† 'Phil. Trans.,' vol. 150, 1860, p. 185; vol. 152, 1862, p. 591.

The procedure adopted was the obvious one of measuring directly the effect of the displacement difference, air to steam, on the weight of the copper sphere; condensation of the steam upon it being prevented by raising it to a temperature above that of the steam before the vapour was admitted into the calorimeter. The calorimeter used was spherical, and 14 cm. in diameter. The sphere was first equilibrated when cold as it hung in the calorimeter, the air in the calorimeter being to a great extent dried by leaving in it a vessel of calcium chloride throughout the previous night. In considering, then, the density of the air in which equilibration was effected, the hygrometric state of the air need not be taken into account. At this point the temperature of the air in the calorimeter and the height of the barometer were observed. Steam was now got up in the boiler attached to the calorimeter, the calorimeter opened, the calcium chloride removed, and the process of heating the sphere begun.

This consisted in applying to it a spirit flame as it was slowly swung round on the suspending wire. Of course the first effect of the flame is to precipitate moisture on the cold metal, but as this grows hot the moisture drys off. To avoid as far as possible a change of weight during this process, due to oxidation, the precaution had been taken of subjecting the sphere to a prolonged course of similar treatment previously, till further heating over a reasonable interval of time had no appreciable effect on its weight.

When the temperature of the sphere all over is well above that of the steam, shown by touching it here and there with a stirring-rod wet with water, steam is admitted into the calorimeter.

It is observable that if now, immediately the calorimeter is filled with steam, the counterpoise be adjusted till equilibrium obtains, this counterpoise will be excessive. The steam is superheated in the vicinity of the copper sphere, and its density diminished below its true density at the prevailing pressure. In a few moments the apparent weight of the sphere diminishes. The change may be as much as a milligram. The vibrations of the balance now become steady, and this state of equilibrium continues for from 10 to 14 minutes. The quantity by which the counterpoise has been increased to maintain this equilibrium is the true result of the experiment. Subsequently a slow and uniform increase in the apparent weight of the sphere takes place, at the rate of 1 milligram in five minutes. This effect, which is considered further on, is apparently due to radiation, and consequent slow continued precipitation of water in the sphere. It does not apparently interfere with the experiment. When the experiment is concluded, I found it necessary sometimes to make a second observation of the height of the barometer.

To reduce the experiment the following values are required:—

The volume, V_1, of the sphere at the temperature, t_1, of the air.
" " V_2, " " t_2 " steam.
" reading, p, of the barometer at the time of equilibrating the sphere in air.
" " P, of the barometer at the time of equilibrating the sphere in steam.
" weight, ϖ, added to the counterpoise during experiment.

From these measurements, if D be the deduced density of dry air at the temperature t_1 and pressure p, then the density, δ, of steam (weight of 1 c.c.) at the pressure P, is got by—

$$\delta = \frac{V_1 D - \varpi}{V_2}.$$

This is a close approximation; for if w_1, v_1 represent the weight *in vacuo* and the volume of the counterpoise respectively, when the sphere is equilibrated in air; w_2, v_2, the weight and volume of the counterpoise when the sphere is in steam; and if d be the density of the air prevailing during this last period, and $W =$ the weight of the sphere *in vacuo;* then first :—

$$w_1 - v_1 D = W - V_1 D,$$

secondly,

$$w_2 - v_2 d = W - V_2 \delta.$$

Assuming $v_1 D = v_2 d$, as the difference between D and d will be small or non-existent, and $v_2 - v_1$ is also small, and subtracting, δ is obtained as above.

The details of eight experiments effected in this way are contained in Table III. It is only necessary to observe regarding the data of these experiments that V_1 and V_2 were based on a measurement of the volume of the sphere made by weighing it in air and in distilled water in the usual way. After all corrections, the volume was found to be 164·60 c.c. at the temperature 10·50. The volume at the temperatures prevailing during the subsequent experiments was in each case obtained from the formula of Matthiessen,*

$$V_{T_2} = V_{T_1}\left(1 + a\,(T_2 - T_1) + b\,(T_2 - T_1)^2\right),$$

where $a = 4{\cdot}443 \times 10^{-5}$, $b = 5{\cdot}55 \times 10^{-8}$.

* 'Roy. Soc. Proc.,' vol. 15, 1866, p. 220.

Table III.—Experiments on the Density of Saturated Steam at Atmospheric Pressures.

	t_1.	V_1.	t_2.	V_2.	p.	D.	ϖ.	P.	δ.
						0·00			0·000
1	19·12	164·67	99·96	165·33	758·90	12066	0·098	758·90	6090
2	10·20	164·60	100·31	,,	768·56	12604	0·1065	768·56	6092
3	10·55	164·60	100·37	,,	770·18	12617	0·1065	770·18	6106
4	8·68	164·58	100·28	,,	768·30	12668	0·1075	767·76	6094
5	8·49	164·58	100·19	,,	765·25	12626	0·1070	765·25	6097
6	7·54	164·57	100·17	,,	765·30	12672	0·1080	764·85	6081
7	8·59	164·58	100·13	,,	763·68	12596	0·1065	763·68	6097
8	8·79	164·58	100·05	,,	761·49	12551	0·10625	761·49	6067
	Mean	values......	100·18	..	..	..	..	765·08	6090

On comparing the result deduced as the mean of these eight observations with the value tabulated opposite the pressure of 765 mm. in Table II, it is seen that the experimental value is practically identical with that derived from Zeuner's formula. From the experiments 0·000609, from the formula 0·000610. I have thought, then, Zeuner's results probably the safest to adhere to of the many estimations that have been advanced for the density of steam at atmospheric pressures.

The method of using Table II is obvious. The volume of the substance estimated in cubic centimetres is multiplied by the suitable value taken from the table. This is the displacement in steam. The displacement in air must also be estimated for the prevailing temperature, t_1, and pressure, by reference to a table of air densities, as the difference of the two is, of course, that which affects the observation of the weight of the substance transferred to an atmosphere of steam.

If it be desired to secure the observations from error as far as possible at all points, then two further corrections on the value of w are necessary :—(1.) An allowance for the change of volume of the substance due to thermal expansion in passing from air temperature to steam temperature. This may be considerable in the case of metal vessels or large masses of metal. This correction is additive to the value of w. (2.) A correction for the displacement in steam of the precipitated water, *i.e.*, the reduction to *vacuo* of the weight of water w. This is also an additive correction.

Both these corrections are included in the following equation for the true weight of condensation, w,

$$w = \frac{w_1 - V_1 D + V_2 \delta}{1 + \delta}, \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (2.)$$

where V_1 = volume of the substance at t_1,
V_2 = " " t_2,
D = density of air at t_1 and prevailing pressure,
δ = " steam at prevailing pressure,
w_1 = the weight added during experiment.

This formula departs from strict accuracy only in so far as it assumes unit mass of water to occupy unit volume at the temperature of the steam.

Approximate Correction for Displacement.—In a great many cases, the vast majority of cases, indeed, it will be sufficient to substitute for the foregoing a far simpler correction based on the density of steam relative to air. Assuming a mean pressure of 760 mm. :—

For air at	0°	the relative density	is	0·00069,
"	5°	"	"	0·00066,
"	10°	"	"	0·00064,
"	15°	"	"	0·00062,
"	20°	"	"	0·00060.

The volume of the substance in cubic centimetres is to be multiplied by the most suitable of these factors to ascertain the amount to be deducted from the apparent weight of condensation.

Correction for the Carrier.—A deduction, from the observed weight of precipitation, due to the calorific capacity of the carrier is of course necessary. This is effected on a previous experiment (or experiments) on the empty carrier and proportionately to the relative extent of the ranges in the two cases.

On the Accuracy of the Method and the Error arising from Radiation.

Many experiments bearing on the accuracy of the method are contained in my former papers. It is sufficient to say here that:—(1.) Successive experiments on the same piece of matter, whether a good conductor of heat or a bad conductor, show one with another a consistency of result exceeding that found in the records of observations by other methods, as in Regnault's experiments, using the method of mixtures on a very elaborate scale, and dealing with very large quantities of matter. (2.) The results obtained, both with good conductors and bad conductors, agree closely with the most reliable determinations of Regnault, Bède, Mallet, &c. (3.) Wide variations in extent of surface, and in the quantity of the substance placed in the calorimeter, fail to affect the consistency of the result. I will explain with one example. A limpid crystal of barytes weighing 76·109 grams, placed in the calorimeter, afforded 0·10923 as the mean specific heat between 9·65° and 100·30°. It was now broken up into small fragments, which were piled up on the carrier: 65·143 grams were thus returned to the calorimeter. These afforded 0·10910 as the mean specific heat over the range 9·60° to 99·80°.

This no mere accidental coincidence.* It is certainly exceptional for repetition experiments to differ by more than half per cent. With ordinary care, indeed, they are quite as faithful and as sure as repetition determinations of specific gravity made in the ordinary way, and on similar quantities of matter.

It is to be added that since the experimental evidence in support of the method was published much has been done, using various modifications of the apparatus. I have found that the results in

* A table containing many such experiments is contained in my paper "On the Method of Condensation," p. 362.

every case were consistent from one apparatus to another. In large or small calorimeters, with varying dimensions of steam-pipe, boiler, or exit tube, the consistency of result was maintained. Thus the single calorimeter described further on will be found very different in the disposition of its parts from the form designed for use within the case of a balance as described in my former paper. In the course of some recent experiments on the mineral sulphides, I desired to check in some cases former results, by dealing over again with the specimens dealt with in the first calorimeter. I have collected these cases here.

	W.	t_1.	t_2.	w.	sp. h.
Galenite, cleavable fragments—					
Former result	56·967	10·50	99·90	0·496	0·05224
Another former experiment ...	56·951	10·98	99·76	0·493	0·05232
" " " ...	56·951	11·77	99·70	0·487	0·05219
Experiment in new calorimeter	56·759	18·05	100·00	0·4545	0·05242
Another specimen, close-grained—					
Former result	87·840	13·27	99·60	0·6955	0·04921
Result in new calorimeter	93·817	17·65	100·10	0·7141	0·04952
Sphalerite, cleavable, high lustre—					
Former result	45·765	11·80	100·18	0·863	0·11445
Result in new calorimeter.....	45·755	16·25	99·80	0·8151	0·11442
Pyrite, two interpenetrated cubes—					
Old result	46·710	10·50	99·90	1·016	0·13111
Recent result...............	46·710	17·50	100·00	0·9375	0·13052

As regards what is probably occurring in the calorimeter, it is convenient to consider the duration of an experiment as divided into a period in which the substance is rising in temperature, and a period in which, having attained the temperature of the steam, it hangs surrounded by an atmosphere of saturated vapour, while its weight is being determined. I briefly epitomise, in part from my former paper, the following considerations. Throughout the first period:—(*a.*) The film of water, which is almost immediately formed upon the substance, is having its outer surface continually renewed by precipitation of fresh steam, and hence presents a surface but slightly cooler than the surrounding steam. (*b.*) This film of water will be very adiathermanous to radiation from water vapour, so that most of the steam will condense, not by radiation, but by contact with the slightly cooler outer service of the water-layer. (*c.*) A reduced pressure will obtain, or tend to obtain, around the substance, giving

rise to an inflow of the surrounding steam. This condition tends to correct what error might arise from radiation from points in the vapour near the surface of the body. Vapour so precipitated will, in fact, be carried by the indraught and thrown upon the substance. In view of this, care is taken so to construct the calorimeter that no sharp cross-draughts play upon the substance on the entry of the steam, which might possibly diminish the protective effect of the indraught. (*d.*) It is possible that during this first period, if the walls of the calorimeter heat more quickly than the substance, some radiation might occur from the walls to the substance, appearing as a minus error in the result. It is to be added, however, with regard to (*d*) that the entire duration of this first period is very short, and that the quantitative results of experiments on substances show complete independence of surface conditions.

During the second period, in which the weighing is effected, the substance is to be considered as surrounded by a medium which, without change of temperature, maintains the inner surface of the walls of the calorimeter uniformly at the temperature of the substance or very nearly so, and which itself acts as a screen very opaque to radiation. So that, so soon as the substance has ceased absorbing energy, the conditions are very favourable to preserve it from the effects of further action from radiation. Nevertheless, there is an amount of radiation effect continuing uniformly during this period, and this on a sensitive balance is perceived by continued observation. This was first pointed out to me by Professor Himstedt, of Darmstadt, who, after the appearance of the papers on this method of calorimetry, kindly sent me the results of his observations on instruments of the types described both by Professor Bunsen and by me. He found the weight of the substance hanging in the steam was not absolutely constant, but was subject to an accretion of some 3 or 4 milligrams in an hour.

As it was desirable to ascertain the cause of this increment and how far it could be reduced, and as, too, it was quite conceivable that occasion might arise when allowance for it would have to be considered, I made a considerable number of protracted observations upon it. It is sufficient to observe here that the effect seems with most probability a radiation effect. It is greater for rough than smooth bodies. Lamp-blacking the substance, or the inside of the calorimeter much increases it. Cooling the outside of the calorimeter, as by spraying cold water upon it, increases it. Calorimeters with double walls, having bright reflecting surfaces, show less increment. For large calorimeters it is less than for small ones. It is uniform or nearly so for however long the experiment is continued. On the other hand, increase of the rate of flow of the steam through the calorimeter does not seem to affect it, which appears to differentiate it from any-

thing like an effect due to mechanically suspended water in the steam. It appears, in fact, to arise from a slow loss of heat from the substance to the walls of the calorimeter, steam, in consequence, condensing on the substance.

The numerical results of my own experiments, effected in a large, single-walled calorimeter of spherical form, 14 cm. in diameter, are such as to afford:—

3 milligrams per hour on a clean, blown glass sphere having a surface of 80 sq. cm.

4 milligrams per hour on a dull surface of platinum of 80 sq. cm.

8 milligrams per hour on a lamp-black coated surface of 80 sq. cm.

These results were obtained using a roomy calorimeter.

From experiments in a calorimeter of the type to be presently described, but somewhat smaller (8 cm. in diameter) and double-walled only on the lower, removable part, I take the following:—

(1.) A clean, but not bright, copper box, cylindrical in form, having an external surface exposed to the steam of 52 sq. cm. nearly, standing in a platinum-foil catchwater, exposing an effective surface of 57 sq. cm.; total 109 sq. cm. of clean metal, showed an increment of from 0·45 to 0·5 milligram per 5 minutes. Thus successive observations of the increment every 5 minutes afforded:—

0·4 0·5 0·4 0·4 0·4 0·5 0·5 0·5.

The inside of the calorimeter in this experiment was smooth and clean. Later I made the experiment of gilding and burnishing the inside surface of the calorimeter. This appeared, however, to make no sensible difference: thus, with all otherwise as above, observations gave the increment per 5 minutes:—

0·5 0·5 0·5 0·5 0·5 0·5.

(2.) A rough block of cryolite weighing 37 grams, resting on the platinum catchwater as above, gave 0·74 milligram per 5 minutes; the observations were:—

0·7 0·8 0·7 0·8 0·7.

(3.) The catchwater of platinum foil alone, with stirrup, as described in (1), in the calorimeter previous to gilding its interior gave the increment, determined every 5 minutes:—

0·2 0·3 0·3 0·4 0·3 0·4 0·5 0·3
0·3 0·4.

Mean increment, 0·34 per 5 minutes.

(4.) The stirrup, which is of silver tarnished to blackness, and the cross wires serving as the platform, without the catchwater, gave :—

0·1 0·1 0·2 0·1 0·2

Mean, 0·14 per 5 minutes.

I have chosen these observations out of a large number of various experiments as sufficient to give a good idea of how far this radiation effect should be considered in observations made with this calorimeter. But before dealing further with this question a few more experiments must be added.

(5.) Frequent observations with sensitive thermometers failed, even under extreme conditions, to reveal radiation across the steam to the walls of the calorimeter. These experiments were conducted in this manner. A very delicate thermometer, removed from a hypsometer, was coated thickly with lamp-black over the bulb, which measured some 4 cm. in diameter. This was arranged so that the bulb occupied the centre of a spherical brass calorimeter, single walled, and left clean on the inside. Diameter, 14 cm. Steam was admitted and the position of the mercury in the projecting stem of the thermometer observed through a telescope. Cold water was now plentifully sprayed over the surface of the calorimeter, but, although an observer attentively watched the thermometer through the telescope while this was being done, no change in the position of the thread of mercury could be detected. All this time the abundance of evaporated water rising from the outside of the calorimeter and the increased drip from the inside showed that heat was rapidly passing through the walls. This experiment is surprising, perhaps, but if the extreme smallness of the effect indicated in the weight-experiments be considered, it need not, I think, negative the suggestion that the increment is a radiation effect.

(6.) It remains to add what is perhaps the most conclusive experiment on this radiation question—the experiment of coating the inner walls of the calorimeter with lamp-black and comparing the rate of increment with the rate obtaining in the absence of this coating. The increase of surface with the lamp-black is the greater, as the many tiny globules of water condensing from the steam and adhering to the inner wall become each coated with the rough black deposit. Steam was first passed through the space between the walls of the calorimeter till all was heated. Had this not been done, the deposit of lamp-black would have been washed away from the inner surface by the copious condensation. When the steam had been some 5 or 6 minutes in the calorimeter, the current through the jacket was stopped and observations begun. It is to be observed that the presence of moisture between the walls would tend to diminish the protective

effect of the jacket, increasing radiation. With everything else as in (1) [*ante*], observations every 5 minutes gave :—

3·4 4·0 3·6 3·0 3·8 3·2.
Mean, 3·5 milligrams per 5 minutes.

(7.) The lamp-black was now removed and the inside cleaned. With the same order of procedure as before exactly, and everything the same except for the absence of the lamp-black, the results are :—

0·7 0·7 0·8 1·1 1·1 0·8 1·0,
or nearly 0·9 milligram per 5 minutes.

The effect of the water deposited between the walls of the jacket is probably seen in the difference between this experiment and the results of (1). The difference existing between the results of (6) and (7) is conspicuous and so far as I can see can only be explained in the hypothesis that the increase is due to radiation. The inference is strong that the ordinary effect is also in some degree, if not entirely, of the same nature.

When in special cases it is thought necessary to allow for this effect of radiation (as I will call it), I would suggest making an observation on the amount of the increment, say over 10 minutes, subsequent to the weighing being completed, and deducting proportionately to the time occupied in weighing—not proportionately to the time since the first admission of steam into the calorimeter, as I do not think it is warranted to assume that the same effect obtains during the period in which the substance is rising in temperature. It is to be remembered, in fact, that (as before observed) an effect of the opposite sign as affecting the observed condensation on the body may then have obtained. It is to be observed that some of the increment will be due to the carrier. This would have amounted to rather more than one half in such a case as (2), considerably more in (1). The ordinary deduction from the total condensation for the calorific capacity of the carrier will eliminate approximately this portion of the radiation increment.

But it may be asked: how is it to be determined at what precise moment the true precipitation has ceased? when has the substance attained the temperature of the steam?

In considering this question it is necessary to realise the nature of the phenomena observed in the course of an experiment. Suppose the 37 grams of cryolite, previously referred to, was being dealt with. Steam is admitted. For three minutes, about, we try in vain to equilibrate the balance. Equilibrium is impossible for the reason that condensation is progressing so rapidly upon the cryolite that so soon as an approximation to equilibrium is obtained this is again

immediately disturbed. The gain is so rapid during this period that weighing, or observation of the rate of increase, is impossible with the ordinary balance. Between this state of things and that prevailing subsequently, when weighing has become possible, there is, of course, no abrupt transition. But, as observed, it is found simply that weighing (in such a case as I am considering) has become possible, probably in the course of the fourth minute. Let the vibrations of the balance be now observed. The oscillations are perfectly regular for about one minute. If the balance reads tenths of milligrams, a slight preponderance of weight will probably be then observable. Watching the vibrations for 5 minutes from the fourth minute, suppose, and then moving the rider till there is equilibrium, the gain is found to be seven-tenths of a milligram. Observing the balance for another period of 5 minutes, the result is again seven-tenths, and so on. However long observation is carried on, the gain is seven or eight-tenths per 5 minutes.

Now this is certainly not due to heating of the substance, and it is established by the observations that the seven-tenths of a milligram represents an uniform rate of increment. But from the fourth to the ninth minute observation gave but seven-tenths. It is safe, therefore, to conclude that by the end of the fourth minute the true condensation due to the calorific capacity of the substance has ceased.

It remains to consider numerically the importance of this source of error in affecting the degree of accuracy attained by this method of calorimetry. I will illustrate this effect by assuming an extreme case in which the increment is entirely ignored, no correction being made for it except the unconscious one made in effecting the deduction for the carrier, and I will suppose that after weighing has become practicable 10 minutes be allowed to elapse, in order to put the temperature of the substance beyond question. I will consider, both in the case of a non-conductor of heat and in the case of a good conductor, the consequent effect on the accuracy of the result.

(*a.*) For the former I cannot do better than take the case of the piece of cryolite referred to in (2). It weighs 37 grams. For a range of 90 degrees I get from an experiment on this specimen (" Specific Heats of Minerals," p. 263) that there would be a condensation of 1·588 grams due to the calorific capacity of the cryolite alone. The increment during the time of observation, according to (2) [*ante*], will be $0·74 \times 2 = 1·48$ milligrams. Observations on the carrier over a similar interval have, suppose, been made once for all. The deduction due to its thermal capacity then reduces the radiation effect by $0·34 \times 2$ or 0·68 milligram; leaving a + error of 0·8 milligram. This is an error of 0·05 per cent., or 1 in 2000, on the specific heat determined.

(*b.*) Let the case be that of a piece of copper having the dimensions

of the copper box referred to in (1), that is, a cylinder 3·65 cm. in diameter × 4·50 cm. in length. This will be about 405 grams of copper, giving through a range of 90° a precipitation of 6·343 grams of steam. The increment in 10 minutes, less that of the carrier, is $0·5 \times 2 - 0·34 \times 2 = 0·32$ milligram. The error introduced by neglecting this is 1 part in 20,000 about.

These figures afford an idea of the extent to which this radiation effect, if neglected entirely, affects the results obtained in these calorimeters. And, in practice, even these are excessive; they double the error actually obtaining, for it will be found that half the interval assumed for observation will be more than sufficient in order to be quite sure of the weighing, and of the condition of the substance when of such dimensions as I have assumed.

What radiation error then can be detected with this method of condensation is not alone in general extraordinarily small, but its amount is easily ascertained, and a close approximation to its entire elimination possible. To the question whether undetected error from radiation or other causes enters into the quantity of steam precipitated upon the substance in the first instance an answer, based on direct experiment, cannot be given. Comparative experiments on substances, using other methods, would not afford a conclusive answer, as the exact extent of the error entering into other methods is at least equally open to surmise. It is to be said, however, that, *a priori*, no grave error is to be expected, and what experimental tests have been applied appear to show that such error if existent must be very small. I have mentioned the general results of these tests.

The Construction of the Steam Calorimeter.

What is the best form to confer upon a steam calorimeter? There are so many conditions to fulfil that the choice is really not very large. It must permit of being filled rapidly with steam, which should preferably descend in the calorimeter, as it then mixes less with the air. Arrangements must be made for allowing a slow current or circulation of steam to continue all the time the weighing is being effected. This must be such as will not interfere with the accurate determination of the weight. If this circulation of steam be stopped at any time there is a minute but definite fall in temperature. A sensitive thermometer will show this. It is about the one-thirtieth of a degree. If the current be stopped when the body is accurately poised, and after an interval be started again, a minute increase of weight is at once apparent. The substance has cooled in the interval and is reheated on re-establishing the current.

Although this current of steam may be quite slow while weighing is proceeding, there must be complete control over it, so that it may

be made rapid at first when air and mist are being swept out, as the object then is to let in the pure steam as quickly as possible around the substance.

The construction at the point where the suspending wire passes through to top of the calorimeter, ascending to the balance, must be such that no rubbing of the wire in its vibration up and down occurs. That is, the wire should hang in the centre of the necessarily small orifice provided for it; as this is a troublesome adjustment, an automatic arrangement should be provided. No condensation of steam must occur on the wire where it passes out, or above that point, and steam must be hindered from passing up along the wire into the balance.

To effect the accurate determination of $t°$, it is important that the temperature in the interior of the calorimeter change slowly. This necessitates that the walls of the calorimeter be fairly non-conducting. They must withal be light, or they will remain hot an inconveniently long while after an experiment, and will take long to heat, which hinders the rapid filling of the calorimeter with steam.

The interior of the calorimeter must be easily got at for drying out and cleaning, and for putting the carrier readily in its place. It should fit together fairly steam tight, and be simple in construction, and so cheaply made.

A sectional elevation to a scale of one-fourth of a convenient form of the calorimeter is given in Pl. 6, fig. 1; I have worked a good deal with it, and have found it fulfils the requisite conditions.

I may observe that, as regards the condition of preserving a steady internal temperature, no form not very cumbersome will confer perfect satisfaction if used in a room in which a rapid variation of temperature is suffered to occur just before an experiment. In every case it will be necessary to carefully screen off the boiler supplying the steam, so that the waste steam and hot gases from the burner pass up a flue or directly out of the room. It would be best of all to locate the boiler in a neighbouring room, taking a steam pipe through the wall. I have myself suffered more from defect in this part of the arrangements than any other, and what discrepancies occur from one experiment to another I attribute to unsteadiness of the initial temperature. I generally find that in the fifteen or twenty minutes during which the boiler is heating, the thermometer in the calorimeter may show a variation amounting to one-tenth, often to one-fifth, of a degree. My practice is to take three readings during that interval, one just before lighting the burner beneath the boiler, a second when steam is up, and a third just before making an experiment, after the steam has been let flow freely out of the boiler up a flue for eight or ten minutes to clear air and mist out of the boiler. I assume the mean of these three as the

mean temperature of the body if a bad conductor. I take the last if a good conductor. In any case the error introduced will be small, but, of course, it is to be avoided.

It is seen in section that the calorimeter is double walled. The carrier and catchwater are shown hanging within it. In form it is cylindrical, the inner cylinder being cone-shaped at each end. The pitch of the upper cone is made so high that drops of water will run down it, and not fall off it. It is of brass, very thin, the inside being gilt and burnished. This is not essential, but keeps it very clean from the sulphur and antimony which are carried from the rubber steam-pipe by the steam. The surfaces between the walls are simply burnished. Outside it is covered with well-shrunken cloth. The steam admission way is in the upper part of the calorimeter, and by this pipe this upper part is securely fixed to the upright supporting the table carrying the balance. The lower part is entirely removable from the upper. It meets it on a well-ground surface, and is secured in its place by an external bayonet catch at either side. Steam is admitted by the brass pipe shown in position at the other side of the upright. This pipe is removable and is very readily laid in its place, being guided by a hollowed-out wooden support attached to the base board of the calorimeter-stand. It is connected by a thick rubber tube, 2·4 cm. in internal diameter, with the boiler. This tube must have a fall the whole way to the boiler to keep it from choking with water. On steam being admitted, it rapidly drives out the air, the steam descending in the calorimeter. To allow the air to escape, a means is provided for opening the lower orifice of the calorimeter fully. This is effected by rotating a milled-headed screw projecting at the side of the stand, and shown dotted in the section. The shaft from this screw passes across the face of the upright board carrying the calorimeter, and is furnished with two projecting arms. One of these is provided at its extremity with a hard-wood cup, shown dotted, in the depressed position. The other carries a conical catchwater of brass with a sloped brass tube attached, shown in position at the lower orifice of the calorimeter. Either of these, the wooden cup, or the cone and tube, may be brought to cover the orifice in the calorimeter by a longitudinal movement (of about 2·5 cm.) of the shaft of the milled-headed screw. Before experiment the non-conducting wooden cup closes the orifice. Just before coupling with the boiler, this is depressed and pushed back. When it is judged from the appearance of the steam escaping at the orifice that all air is expelled, the cone and tube are elevated against the orifice, closing it except for a slow current of steam still free to issue from the sloped tube. The water draining from the calorimeter also issues through this tube, falling into a dish placed to receive it.

The thermometer for reading the initial temperature is inserted in

the fixed upper part of the calorimeter. It is at a convenient slope for reading, and its bulb penetrates into the calorimeter till just over the substance placed on the carrier. In careful work it is well to read the thermometer by a telescope. A very accurate, but less convenient, way is to read with a lens, which is moved about till the image of the last graduation on the stem of the thermometer reflected in the thread of mercury is seen to be covered by the graduation. There is then no parallax error. The thermometer is withdrawn just before letting in steam, and the tubulure plugged with a small cork. After the weighing is finished, a thermometer for reading the boiling point may be inserted in this tubulure. Previous to the admission of steam, the tubulure taking the steam-pipe is kept closed by the stopper of wood overlaid with cloth, shown dotted in its position. The side pieces which go towards supporting the table for the balance are cut out as shown at each side, so that the operator can see to remove the stopper, and insert the steam-pipe rapidly. My practice is to pinch the rubber tube conveying the steam for the moment in which the steam-pipe is being laid on; when in position it is released, and steam let flow into the calorimeter. The steam-pipe is in part covered with thick baize, so that it may while hot be grasped by the hand.

A section, fig. 2, Pl. 6, full size, shows the arrangement adopted to render the wire in its passage through the roof of the calorimeter self-adjusting in the centre of the orifice provided for it, or rather to render the orifice self-adjusting on the wire. The coned roof of the inner wall of the calorimeter is carried through the external cylindrical jacket, flanged at the top, and ground smooth. A loose coned piece also with ground flange rests on this. The upper end of this cone is turned down to a knife edge, and just brought flat on a fine stone. On this a tiny disk of copper or brass drilled centrally with an orifice about two-thirds of a millimetre in diameter is laid looosely. The wire bearing the carrier is brought through this disk, which weighs about 22 milligrams. Above the disk is placed a spiral of fine platinum wire held in a forceps, which by two binding screws may be put in circuit with a battery. Through this the wire also passes. Finally, before the wire rises into the balance, it is embraced by an inverted cone (fig. 1), turned in hard wood, which is adjustable in position, being held to the under face of the table by two spring clips, as a slip is held on the stage of a microscope. On the table a balance, not shown in the figure, stands. The wire ascends to the left arm of this balance.

The adjustment of the suspending wire is very obvious. The balance is set so that the wire hangs freely through a large aperture in the table provided for it. The inverted cone and the small cone on the calorimeter are next set to let the wire pass centrally. The

aperture in the inverted cone is about 2 mm. in diameter, that in the lower cone about 3 mm. Their adjustment, therefore, does not present any difficulty, and once made need seldom be disturbed. The disk resting on the lower cone is permitted to adjust itself. During an experiment, it is kept warm by radiation from the platinum spiral, which is put in circuit with a storage cell. It thus remains dry and quite free to move about on the knife edge of the cone. As the wire swings about, it carries it with it from side to side. Finally, when the amplitude of the vibrations diminish sufficiently, it leaves it correctly adjusted, for, of course, the swinging wire will always so shift the disk as to swing in a diameter of the orifice. This arrangement I have found to act very perfectly. The adjustment of the wire in the old arrangement was very troublesome; it demands no attention with this, which in no way interferes with the weighing. The disk should not be lighter than the weight specified, for if too light the amount of steam pressure which it is necessary to maintain will raise it at one side.

It is remarkable as regards the platinum spiral for maintaining the orifice dry that an error may be introduced if this is kept at too high a temperature. It apparently then sets up an ebullition of the water precipitated on the upper part of the calorimeter, the result being a splashing or rain upon the substance hanging below. I have been led to suppose this by observing that the radiation effect is apparently increased by heating the spiral excessively. On the other hand, too cold a spiral, of course, also causes error by permitting water to condense on the wire both above and below the orifice. The right temperature seems to be that which gives to the spiral a just visible red when steam is *not* in the calorimeter. The effect of the up draught of steam is to cool it.

The suspending wire should be of platinum; about 0·1 mm. diameter will be sufficiently strong for most purposes. This ascends to the left end of the balance beam, being directly attached to a counterpoise equilibrating the right-hand pan.

It is well to load the balance till there is equilibrium when the empty carrier is in position. The counterpoising of the substance before an experiment then affords its weight, with, of course, the ordinary correction for air displacement, if thought necessary.

The balance used by me is a Sartorius short-beam (14 cms.). The cheap form of this instrument answers admirably, reading accurately to tenths of milligrams when loaded with over 100 grams. It will do so, I believe, up to 200 grams. It is quick, and in every way is perfect for the purpose. On removing the pan stops the suspending wire may be taken through the drilled aperture left in the plate-glass base.

The stand of the calorimeter is of well-seasoned mahogany, strongly

fitted together. To enable it to be levelled, it is supported on two levelling screws in front and a centrally placed foot at the back. To afford more vertical room, the base board is cut out centrally in front. The carrier for supporting the substance within the calorimeter is shown in position within the calorimeter. It is made of silver wire, about 0·5 mm. in diameter. The catchwater is of thin platinum-foil, and is removable for drying and cleaning. It is, in fact, supported on a projecting claw beneath the ring of the carrier. Across this ring fine platinum wire is stretched, forming a platform on which the substance may be laid. Four wires crossing at the centre will in general be sufficient. When resting on these the substance is exposed to the steam on all sides. The total weight of the carrier is just 3 grams. It condenses about 0·031 gram through a range of 90° C.

The claw supporting the catchwater performs a double function. In the case of a smooth body, which is also a good conductor of heat and of large thermal capacity, such as a thin vessel filled with water, the precipitation is so copious and sudden that it reaches the catchwater before it attains steam temperature. The result is a secondary precipitation on the outside of the catchwater. This might be in some cases so plentiful as to drop from the bottom of the catchwater, and so be lost. The claw serves to entangle this, retaining it on the balance.

It is important that an ample supply of steam should flow into the calorimeter on connecting it to the boiler. To make certain of this, a strong gas-burner and a large boiler should be used. The supply, indeed, should be considerably in excess of what passes up the connecting-tube. If this is not so there is risk of air entering the boiler on first coupling it with the calorimeter, which, mixing with the steam, causes a mist of the cooled vapour to flow up the tube. The danger of this is considerable, as there is a strong tendency to an indraught at the boiler, owing to the buoyancy of the water-gas in the ascending tube. In some experiments on the value of the radiation effect, before alluded to, this came strikingly to my notice. The boiler was fitted with a pressure-relief arrangement, consisting simply of a tube taken externally from the top of the boiler, bent twice at right angles and brought downwards, so that it opened at a level below the bottom of the boiler. Thus a certain small pressure of steam in this was necessary to drive the buoyant gas down this escape-pipe.

Hence I concluded that the continued appearance of steam escaping at the relief-pipe was a sufficient indication of an excess of internal pressure. However, in my experiments a large and unaccountable increment to the weight of precipitation on the substance prevailed, and this I traced after much trouble to the entry of air at the relief-pipe. There was, in fact, a circulation of air and steam within

the tube and boiler; air entering and flowing along one side of the pipe, steam issuing along the other. On narrowing the opening of the tube, so that a well-defined current of steam having the full section of the tube issued, the effect disappeared. The foregoing method of providing for the exit of the surplus steam is defective and unsafe. It is preferable to use a non-return valve of some sort. I find the simple arrangement shown on the boiler in Pl. 7 (scale one-tenth) very effective. It is simply a balanced flap-valve, meeting the vertical exit-tubulure on a ground edge brought fine, so as not to stick with precipitated water. The counterpoise to the weight of the flap can be placed at discretion in one of several notches near the end of the beam, so that the pressure may be increased or diminished. To keep this valve from falling into vibration, the pivot on which it turns bears at one end against a screw, which may be tightened so as to retard a little the oscillatory motion of the beam. The pressure maintained should be small, as pressures appreciably in excess of atmospheric pressure interfere with the working of the calorimeter. The valve should therefore be so set that it just falls shut readily when steam is not issuing. This, according to measurement, in the case of the valve used by me corresponds to rather less than a pressure of 1 mm. of water. The material of the boiler is copper, tinned within. The burner is a large "solid flame" of Fletcher.

Method of Carrying out an Experiment.—I will suppose the calorimeter dry and cold, and ready for the introduction of the substance. This is placed on the carrier which hangs within the calorimeter dependent from the balance. Having adjusted the substance centrally on the carrier, and so that there is no fear of water dropping from any protruding point of the substance over the edge of the catchwater, and so escaping estimation by the balance, the thermometer for taking the initial temperature of the substance is to be inserted in its tubulure. This is done now, before closing the calorimeter, in order to see that its bulb does not strike against the substance or the stirrup of the carrier, and that it is inserted sufficiently far to be well over and close to the substance. The substance should now be roughly counterpoised by placing weights in the right-hand pan of the balance, and before the final adjustment of the balance, the calorimeter closed. On now finally adjusting the equilibrium of the balance, we observe if the wire swings freely through the several orifices through which it passes. The weight placed on the right-hand pan affords W, the weight of the substance, if, as should be arranged, equilibrium be previously obtained between the carrier and the pan. It is necessary also to see that the lower orifice of the calorimeter is closed with the wooden stop, and that the entrance-way for the steam-pipe at the back is also stoppered.

After these preparations the calorimeter must be left a sufficient

time to ensure that the thermometer and substance are uniformly at one and the same temperature. This interval of course varies with the nature and mass of the substance. In accurate work sufficient interval must be left to leave no doubt on the matter, the room being one not subject to sudden variations of temperature. When the required interval has elapsed, the thermometer is read by a hand lens, or better through a telescope. The temperature is noted down. The burner is now lighted beneath the boiler, all hot gas and steam being arranged to pass directly out of the room, as already mentioned, and direct radiation carefully screened off from the calorimeter. When the water is boiling, a second reading of the thermometer is taken and noted down. The boiler is now suffered to pass steam through the coupling-tube for about ten minutes, to ensure that all is free from air and mist. During this time it is better that the tube be directed so that the steam escapes up a flue or out of the room. The interval is to be utilised in checking the equilibrium of the balance, noting down the position of the rider, and observing if the valve on the boiler is working freely and without vibration. At the expiration of the interval, a third reading of the thermometer is taken and rapidly noted. It is then carefully withdrawn from the calorimeter, laid aside, and its tubulure stoppered with a little cork kept for the purpose. Everything is now ready for admitting steam. The steam-pipe, with its nozzle held upwards, is laid out along the slanting board which supports it between the boiler and the calorimeter. The nozzle is then grasped in the right hand, the steam-jet being still directed upwards. With the left hand we turn the thumb-screw commanding the exit tubulure at the bottom of the calorimeter, opening it to the full. The stopper closing the entrance-way to the rear of the calorimeter is now to be withdrawn, and then bringing the left hand back to the rubber steam-tube, we pinch it sharply at a convenient point, some 20 cm. below the nozzle, which, while the escape of steam is thus for the moment prevented, is run into its position. The steam tube is instantly released, and we give our attention to connecting by a switch the platinum spiral with the storage-cells. This only takes a moment, but by this time the steam is already pouring out at the exit way. For thirty or forty seconds it should be permitted to flow out freely; what little condenses on the surrounding objects dries off quickly, and does no harm. The air being thus completely cleared out, the outflow of steam is moderated by closing the smaller tubulure against the exit-way, and in from one to three or four minutes, according to the nature and quantity of the substance, the weighing may be effected.

It will be at once seen by the balance if the substance is completely heated or not. If it is, it will be found that the vibration of the

pointer continues symmetrical, gaining only imperceptibly, perhaps, one division to the right in about five minutes. This will represent about half a milligram. Observed now for another five minutes, a similar addition will be needed. This increment is due to radiation, and the milligram thus accruing during the ten minutes is not to be included in the value, w_1, which we now have obtained.

The experiment is now concluded. It is well, after disconnecting it from the boiler, and while the calorimeter is still hot, to dry it out, as the residual heat then completes the drying very thoroughly. The corrections on w_1, necessary to convert it to the value w of the final equation have already been considered.

The Differential Steam Calorimeter.

In the use of the apparatus just described, it is certain that a high degree of accuracy is attained. In the thermometry and in the estimation of the weight of steam condensed upon the substance, an accuracy of one part in one thousand may, I think, generally be attained. There remain certain causes of error unknown in value within limits, as in all calorimetric methods. In other methods the substance is transferred from a region at one temperature to a region at another. The error of transference has in these cases to be considered. The movement of the substance, when at its highest temperatures, through the air, may be a source of serious error; nor can this error be indefinitely diminished, for a too close approximation of the heater and the cooler causes a transference of heat between the two, in itself a source of error. In the method of mixtures a further source of error is to be found in the continued radiation of the calorimeter and evaporation of the water contained in it.

In the method of condensation the substance is not moved, but the medium around it is changed. Is there any error comparable with the error of transference in other methods? There doubtless is some error, but it is not to be expected that it is at all as great. The error in this case is simply the radiation of the approaching vapour to the substance. Now the velocity with which the steam can be brought to fill the calorimeter is very great, and the momentary radiation of the advancing steam upon the substance is in part compensated by the precipitation of the most active part of this radiating steam upon the substance.

Radiation between the substance and its precincts now begins at a certain rate, the value of which when the temperatures become steady can easily be estimated. The first error is a minus error, the second a plus error. There may be also some minus error of the second sort. The errors thus tend towards a balance. All experimental work indicates that the resultant error is very small. Experiments

in which the surface extent of the same substance is varied considerably more especially point to this conclusion. If these are to be taken as conclusive, the error must be most generally less than the proportion one in a thousand. The variations in successive experiments are of about this magnitude, but are not in any special direction, and so point to no source of error in particular. These experiments are on conductors. (See "Method of Condensation," p. 362.)

The change of medium around the substance from one of greater to one possessing less buoyancy affects the balance and has to be allowed for by calculation based on experiment. It is improbable that more than a very small error arises from this source. Special cases may, however, arise in which this may not be so; and it was chiefly to avoid error from this last source that, in dealing with the large spheres of thin metal, used in the determination of the specific heat of air at constant volume, I resorted to the use of the differential calorimeter.* This has the further advantage of eliminating the radiation error affecting observations in the simple calorimeter. Nor need the thermal expansion of the substance any longer be considered. I describe the apparatus briefly here, as it seems in fact to eliminate to a great extent, if not entirely, what small risk of error obtains in the previously described apparatus. Its advantages, however, are most conspicuous in the use for which it was designed, the only use to which, up to the present I have applied it—the calorimetry of gases. Its application to this branch of physics must ever be its most important use. Indeed in the conditions obtaining in the calorimetry of solids or liquids it is hardly called for. However, even in these latter cases, where it is advisable to enclose the substance from contact with the steam, the differential calorimeter would enable us to effect the experiment somewhat more accurately than would be possible with the use of the single calorimeter.

Plate 7 shows, in side sectional elevation, fig. 2, and front sectional elevation, fig. 1, to a scale of one-tenth, the differential calorimeter which I have at present in use in dealing with gases. The spheres, one of which is used to hold the gas, are shown hanging in the calorimeter. The drawing needs little explanation after what has been said about the single calorimeter. The principle is obvious. Apart from its special application to gases, it may be said that the calorimeter is so constructed that carriers depending from both arms of the balance are hung within it, side by side and only a few centimetres removed from one another, the balance used being a short-beam balance. The substance may be enclosed in a receptacle of thin platinum or copper, with a screwed, air-tight lid, and placed upon one carrier; a similar receptacle, also

* "On the Specific Heats of Gases at constant Volume" (Preliminary Note), 'Roy. Soc. Proc.,' vol. 45, p. 33.

closed air-tight, permanently, if desired, is placed in the other carrier, but containing air only. The receptacles have been previously calorimetrically compared, and adjusted to have the same thermal capacity as they have the same external volume. Such receptacles are most conveniently constructed for dealing either with liquids or solids in small fragments. In these cases narrow-necked vessels, such as may be closed air-tight without difficulty, may be used. The calorimetric adjustment may of course be effected by inserting a calculated weight of copper or other substance of known specific heat in the vessel deficient in calorific capacity, or the difference may be allowed to remain and the constant recorded.

The thermometer enters the calorimeter midway between the carriers; and, as the calorimeter is of good conducting metal enclosed in an outer shield of wood, a uniform temperature may be assumed to prevail after some considerable interval of quiet has elapsed. Now, whatever this temperature, or the temperature of the steam, the receptacles and carriers alone are without effect upon the balance. Nor will there be any increment perceived due to radiation.* If one receptacle, however, contain a substance the balance will indicate a precipitation due solely to this substance. It is to be supposed that any error of transference of media will affect each receptacle alike, subsequent radiation will also affect them alike, and, as their volumes are identical, the varying buoyancy of the media is without effect on the balance.

I have said the precipitation will be due solely to the substance. Evidently, however, in very accurate work this cannot quite be assumed. In fact the thermal capacity of the air expelled from the one receptacle on the introduction of the substance must be considered. The precipitation due to this weight of air, considered as possessing the specific heat of constant volume, must be added to w, the observed effect upon the balance. As the air in these receptacles cannot be considered as dry air, this specific heat may be taken as having approximately the value 0·176. The volume of the substance must then, as with the use of the single calorimeter, be estimated, and the weight of air occupying this volume at the prevailing pressure and temperature calculated. For the pressure 760 mm., and temperature 10° C., if the steam temperature be assumed as 100° C., the addition to w is 0·000037 gram per c.c. of volume occupied by the substance.

* To secure this result, I have found—what might be expected—that a similar condition of surface is necessary. With platinum vessels there would probably be little difficulty in attaining this; but with copper vessels I have found it necessary to keep both surfaces very free from grease and oxidised all over. Washing in ammonium hydrate and heating over a spirit flame seems to bring the surfaces to the desired uniform and permanent condition.

This will afford an idea as to the desirability of making the correction in any particular case. It is evident, too, that this additive correction, 0·000037 gram per c.c., might without further calculation be assumed as the correction in many cases, except very great accuracy be sought.

The construction of the differential calorimeter, it will be seen from Pl. 7, differs from that of the single calorimeter, in being single walled, and having a removable box-like covering of wood, fitted on over all, through which the thermometer is inserted, and which is placed in position after the calorimeter is closed. It is left on throughout the experiment. The obvious use of this box is to favour equilibrium of temperature throughout the calorimeter. Steam is admitted centrally, led from the boiler through a thin brass tube. The diameter conferred upon this tube in the calorimeter figured in the plate might with no disadvantage be reduced somewhat from that shown. This steam tube is in three lengths: an elbow piece fitting into the boiler; a straight horizontal piece of any required length; and finally a vertical double-tee piece. This last is capable of a rocking movement about the axis of its lower horizontal member, so that the upper horizontal member may be shifted either to a central position behind the calorimeter, when it is in the position for throwing steam into the calorimeter, or to one side, when it no longer communicates with the interior of the calorimeter. As drawn, it connects boiler and calorimeter. It is further necessary to provide the means of filling the whole steam pipe before filling the calorimeter. For this purpose the upper horizontal member of the double-tee tube is furnished with two swinging valves, closing it at each end. Before an experiment, and when the tee tube is turned to one side, the outer one of these is drawn inside for a couple of minutes till the whole steam way has been thoroughly cleared of air and heated throughout. During this time the rear orifice of the calorimeter is closed by means of a stopper of cork, bound in soft cloth. When the steam pipe is heated as described this is withdrawn, and the pipe simply shoved across till it comes to a stop provided. In this movement an automatic action lifts the inner valve, shoving it completely to one side, so that there is free way into the calorimeter. This, as will be readily understood, is effected by arranging that the edge of the hanging valve strikes against the projecting tubulure of the short steam way leading into the calorimeter. Subsequently, on inclining the tee piece to one side, the valve resumes its old position, closing the steam pipe. By this arrangement there is little or no leakage of steam, and the operation of turning steam into the calorimeter is effected by one movement of the hand.

There is but one exit way to the calorimeter. This is placed centrally at the bottom. Before experiment this is closed by means

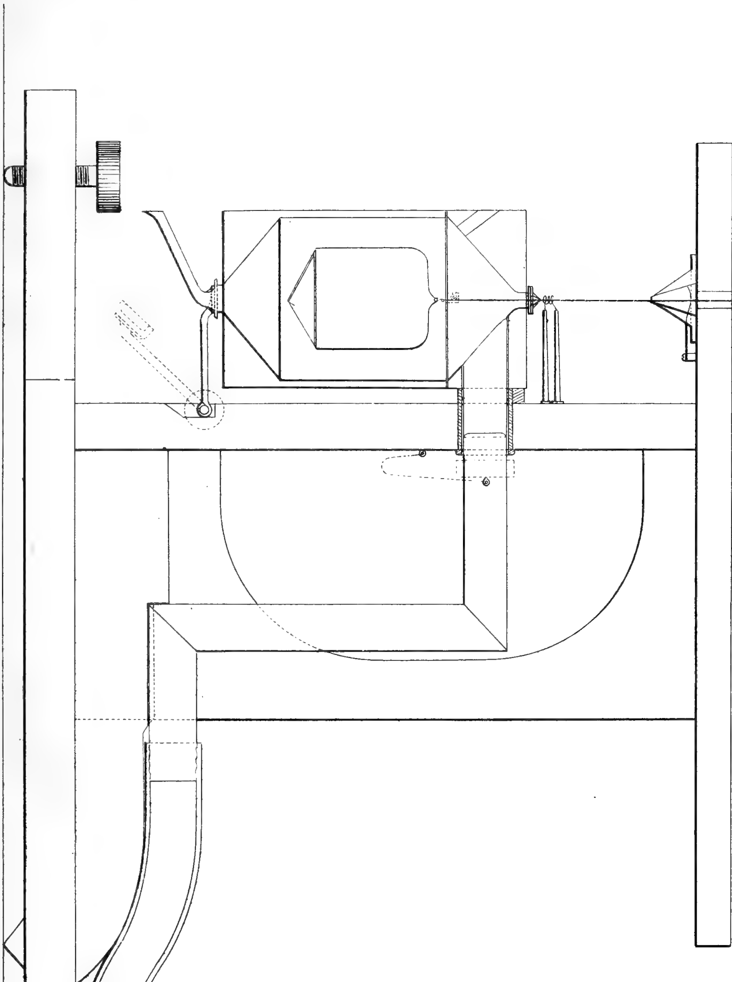


Fig. 1. Scale, One fourth.

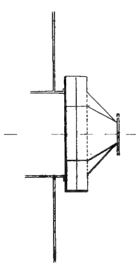
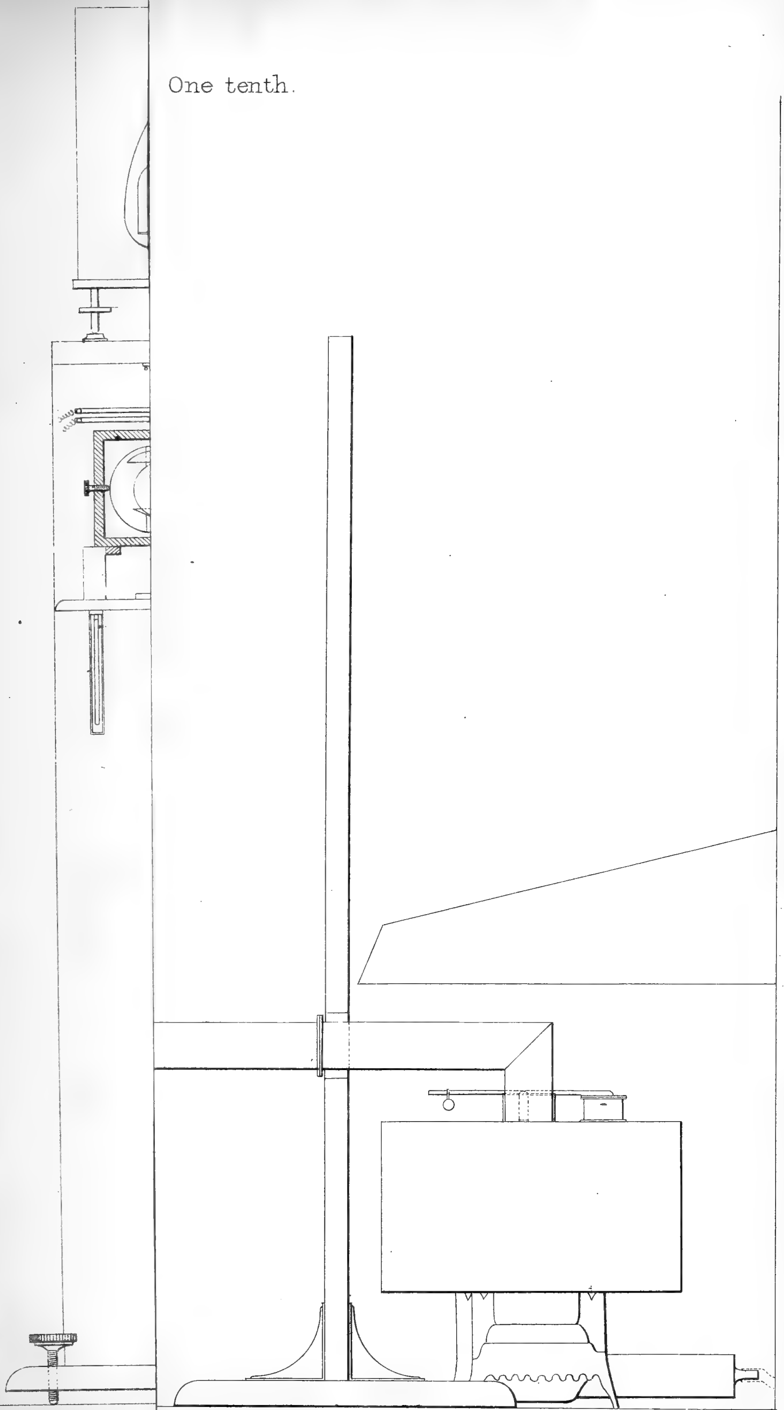


Fig. 2. Full size.

West, Newman lith.



West, Newman lith.

Scale, One tenth.

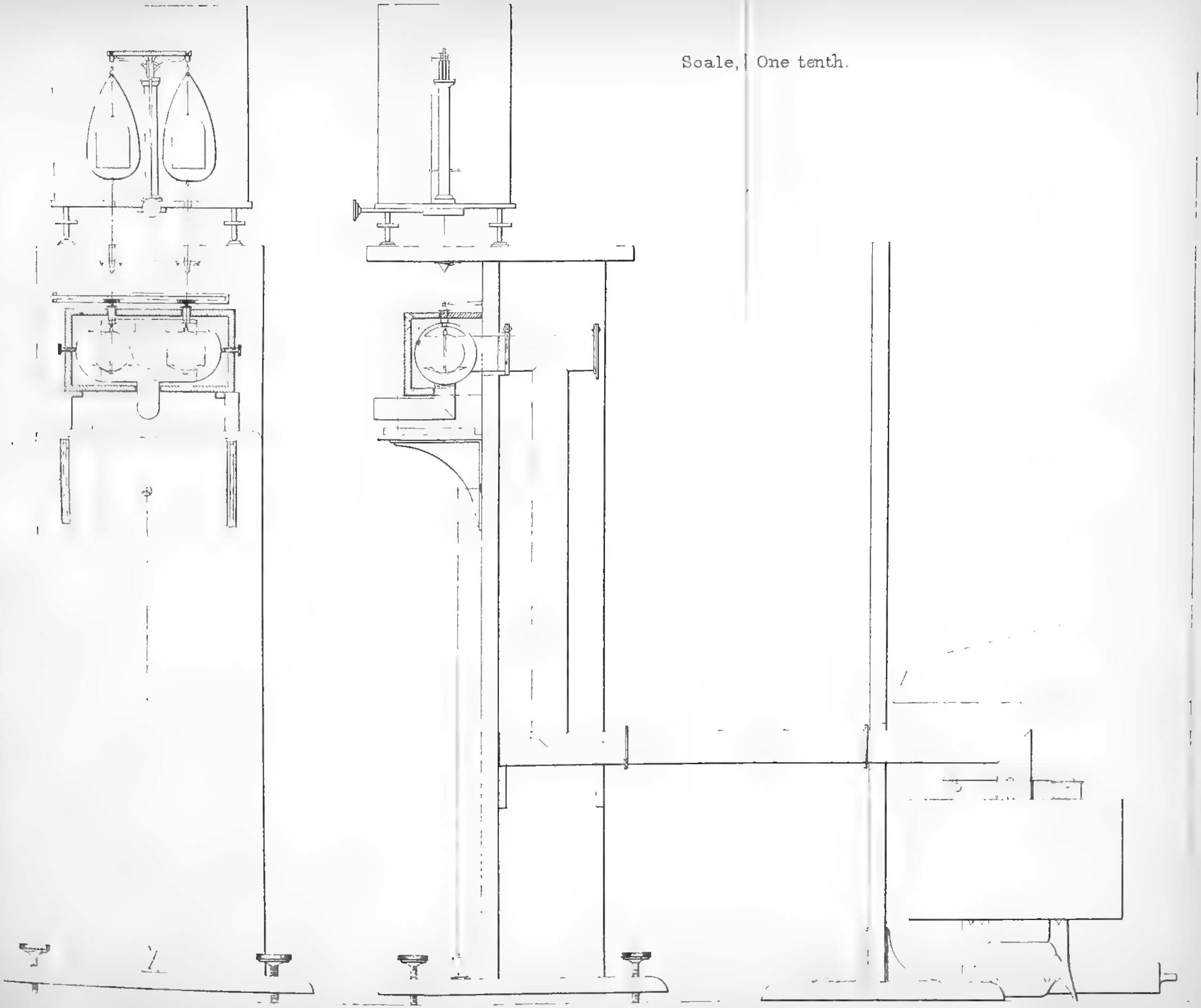


Fig 1

Fig 2

West Newman lith

of a cork; this is removed when steam is first admitted. When the calorimeter is thoroughly filled it is replaced by a second cork, pierced by a short brass tube having a bore of about 7 mm. The diameter of this exit tube might also, with advantage, be reduced from that shown.

The platinum spirals used in drying and warming the suspending wires at their points of exit are in series, of the same length and fineness. The automatic adjustment at the orifices is applied, and, indeed, alone renders the differential arrangement workable. There is no difficulty in effecting the most accurate observation of weight.

The body of the calorimeter is cylindrical in form—a form used for its stiffness and inexpensiveness. The ends of the cylinder are closed by hemispherical caps of thin, spun copper. These have a swelled flange fitting smoothly over the ends of the cylinder, and are further kept in their places by two thumbscrews, fixed in the box covering the calorimeter, one at each end, and so located that when the box is in position and the screws screwed in, they will bear against the hemispheres. Owing to the shape of the calorimeter, it is necessary to shelter the carriers from drip by guards or umbrellas of thin Dutch metal, sprung on two projecting wires into the tubulures of the orifices, so that they can be removed and dried.

To allow of weights being placed on either side of the balance, the ordinary stirrups must be removed. The arrangement shown on the figure works well. Weights may be laid on or taken off the little inner pans without swinging the suspending wires supporting the carriers.

In conclusion, it may be worth remarking that many laboratory-instruction, or even lecture-table experiments of interest may be readily shown with the differential calorimeter. Thus the law of atomic heats is illustrated by placing quantities of two simple bodies, proportional to their atomic weights, in the calorimeter, and equilibrating by weights placed in the upper pans. On admitting steam, the equilibrium of the balance will (theoretically) remain undisturbed. Similarly Wœstyn's law of the constancy of specific heats of bodies in the free and combined states might be illustrated by placing the free elements in the proportions of chemical composition on one carrier, and an equal weight of the combined elements on the other.

The application of the vapour calorimeter to the determination of latent heats of vaporisation is very probably possible. I regret that neither this application of it, nor the allied question of the employment of other vapours besides that of water, can be considered here, the study of the capabilities of the steam calorimeter and a couple of its applications having occupied my time up to the present.

"A Milk Dentition in *Orycteropus*." By OLDFIELD THOMAS, Natural History Museum. Communicated by Dr. A. GÜNTHER, F.R.S. Received December 12, 1889—Read January 9, 1890.

[PLATE 8.]

Of the few Mammalia in which no trace of a milk dentition has been found, *Orycteropus*, the Aard-Vark, has always occupied a prominent place, owing partly to the peculiar structure of its prominent teeth, and partly to its very doubtful systematic position.

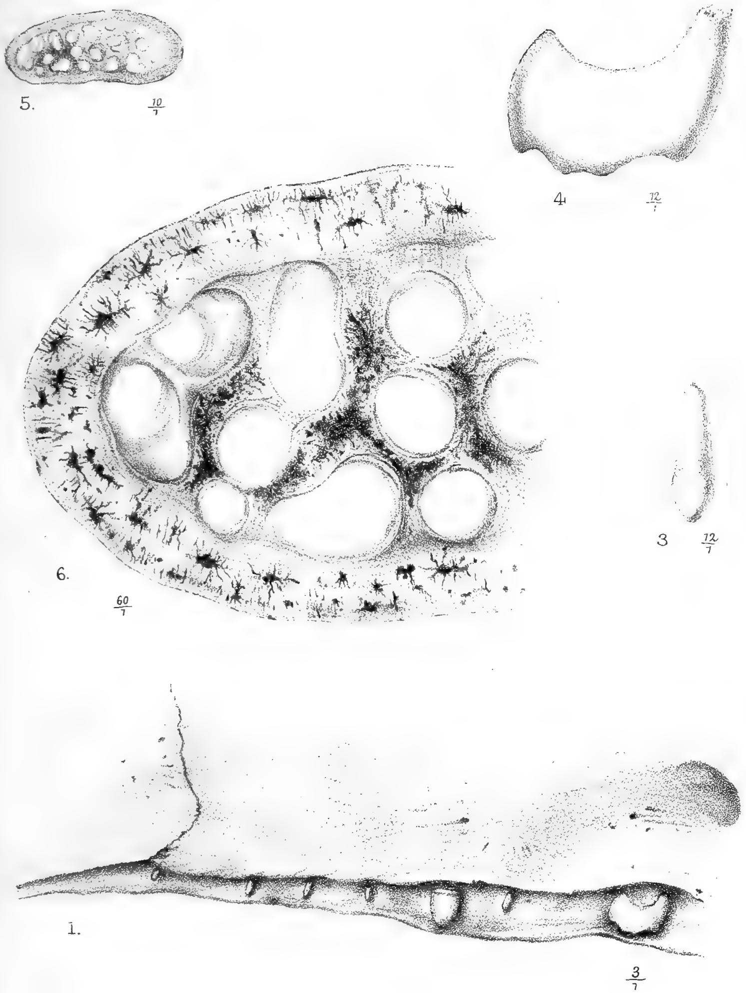
An opportunity has now fallen in my way of proving that it has after all two sets of teeth, those of the first, or milk set, being rudimentary, and probably quite functionless, but nevertheless so far developed as to be all completely calcified, and to be for the most part readily distinguishable by form and position from those of the second or permanent set.

Among the collections in the Natural History Museum there are two very young females of *Orycteropus afer* in spirit, presented by Sir Richard Owen, and it is in these that the milk teeth now to be described occur. The larger of the two measures 18 inches in total length, and the smaller 14 inches.

Each of these specimens has a complete, although rudimentary, set of milk teeth, extending the whole length of the maxillary bones above, and along a rather shorter portion of the mandible below. None, however, are observable in the premaxillæ, or in the corresponding anterior part of the mandibles. The teeth are all quite minute, and it is very doubtful whether they would ever have cut the gum. Specimens rather older than those before me are needed to determine this point.

In the upper jaw there appear to be normally no less than seven milk teeth (Pl. 8, fig. 1). Of these the most posterior (figs. 4—6) is by far the largest, has a rudimentary crown, and two distinct roots, anterior and posterior. The second, proceeding forwards, is far smaller, and is simple and styliform. The next, the third from the back, is also simple, but is far larger in section, and its base is not closed up in either of the specimens; on this account there seems to be just a possibility that this particular tooth is not a milk tooth at all, but only the tip of one of the smaller anterior permanent teeth,*

* These small anterior permanent teeth fall out before the animal is adult, and are absent in the great majority of Museum specimens. There is, however, an immature skull in the Cambridge Museum which shows the alveoli for no less than ten teeth above (at least on one side) and eight below, some of the minute styliform teeth belonging to these alveoli being still in position. For the loan of this skull I have to thank Mr. J. W. Clark, director of that Museum.



Berjeau & Highley del. et lith. West, Newman imp.

MILK-TEETH OF ORYCTEROPUS.

which it resembles closely in size; on the other hand, however, its complete calcification is in marked contrast to the soft condition of the other permanent teeth, and therefore it seems safer for the present to call it a backward milk tooth rather than a precocious permanent one. This question again will be easily settled when rather older specimens are available for examination.

In front of the three posterior teeth there are normally four very minute styliform ones, similar to, and equidistant from, each other, the most anterior placed close to the premaxillo-maxillary suture. Their shape is as shown in fig. 3. On one side of one of the specimens, however, there is an additional minute tooth near the suture, so that there are eight, instead of seven, milk teeth present in the jaw.

Below, the dentition appears to be in a rather more advanced state of development, so that in the larger specimen the germs of the permanent teeth are distinguishable as well as the milk teeth (Pl. 8, fig. 2). The latter are here apparently only four in number; the posterior one, as in the upper jaw, is large and two rooted, and is placed directly over the germ of what appears to be the fourth tooth from the back in the adult animal. The three teeth in front of this large one are minute, pointed, about equidistant from one another, and apparently placed in relation to the permanent teeth as is shown in the drawing. Between the two most anterior of these teeth there is a larger one, equally elevated in the jaw with them, but as yet quite uncalcified, and therefore no doubt merely the tip of one of the small anterior permanent teeth.

As to the structure of the milk teeth, a horizontal section of the last upper one, ground down in the dry state, presents the appearance shown in figs. 5 and 6. The numerous large openings seen in the sections are obviously the sockets into which pulp-papillæ have extended, and it is evident that if further material were available, and the teeth were properly prepared and cut into sections, a commencement of the remarkable histological structure characteristic of the permanent teeth would be found in the earlier dentition.

Since then it appears that the three large posterior teeth of *Orycteropus*, already distinguished by their more molariform shape, do not have milk predecessors, while all the small teeth anterior to them do, and in addition the last milk tooth is markedly different from those in front of it, we ought apparently no longer to look upon this animal as homodont, but instead to consider it as an originally heterodont form in which the incisors and canines have been suppressed to allow free play to the mobile vermiform tongue.

But important as a knowledge of the presence of a milk dentition in *Orycteropus* is, it does not at present render any easier the difficult questions as to the phylogeny and systematic position of that animal.

Although called an Edendate, it has always been recognised as possessing many characters exceedingly different from those of the typical American members of the order.* It has in fact been placed with them rather on account of the inconvenience of forming a special order for its reception than because of its real relationship to them. Now, as they are either altogether toothless or else homodont and monophyodont † (apart from the remarkable exception of *Tatusia*‡), it seems more than ever incorrect to unite with them the solitary member of the *Tubulidentata*, toothed, heterodont, and diphyodont, and differing from them in addition by its placentation, the anatomy of its reproductive organs, the minute structure of its teeth, and the general characters of its skeleton.

But if *Orycteropus* is not genetically a near relation of the Edentates, we are wholly in the dark as to what other Mammals it is allied to, and I think it would be premature to hazard a guess on the subject. Whether even it has any special connection with *Manis* is a point about which there is the greatest doubt, and unfortunately we are as yet absolutely without any palæontological knowledge of the extinct allies of either. *Macrotherium* even, usually supposed from the structure of its phalangeal bones to be related to *Manis*, has lately proved§ to have the teeth and vertebræ of a Perissodactyle Ungulate, and one could not dare to suggest that the ancestors of *Manis* or *Orycteropus* were to be sought in that direction. Lastly, as the numerous fossil American Edentates do not show the slightest tendency to an approximation towards the Old World forms, we are furnished with an additional reason for insisting on the radical distinctness of the latter, whose phylogeny must therefore remain for the present one of the many unsolved zoological problems.

* On this subject see especially Flower, " On the mutual Affinities of the Animals composing the order Edentata," 'Zool. Soc. Proc.,' 1882, p. 358, *et seqq.*

† I have had the opportunity of examining specimens, apparently of a suitable age, of *Bradypus*, *Cholœpus*, and *Dasypus*, and can find in them no trace of a milk dentition, while in each case the tips of the permanent teeth are already formed. So careful has this examination been that I feel sure none of these genera ever have calcified rudiments of milk teeth, although the possibility remains that uncalcified germs of such teeth may be present in still younger specimens ; and these may yet be discovered by means of section-cutting and thorough microscopical search, a method that I hope will be employed by anyone having the opportunity of doing so. Nor can I find any rudiments of calcified teeth (which would in that case be of the permanent set) in a young specimen of *Manis*.

‡ The tooth-change of this Edentate is so peculiar, so very different from that of all other Mammals, including *Orycteropus*, that it has been supposed to be a recently acquired and not an inherited characteristic at all. Its presence is, therefore, no evidence of a near relationship between *Orycteropus* and the true Edentates.—*January 3rd*, 1890.

§ See Osborn, 'American Naturalist,' vol. 22, 1882, p. 728.

"On the Effect of the Spectrum on the Haloid Salts of Silver." By Captain W. DE W. ABNEY, C.B., R.E., D.C.L., F.R.S., and G. S. EDWARDS, C.E. Received November 26,—Read December 12, 1889.

In 1881 one of us gave, in the 'Proceedings of the Royal Society,' a paper with the same title as the above. Since then, however, he has been able to work out a more exact means of measuring the effect of the spectrum on these salts of silver, and it is our desire now to lay the improved results before the Society.

In January, 1887, one of us read a paper before the Photographic Society of Great Britain, "On the mode of measuring Densities of Photographic Deposits, with some remarks on Sensitometers," and in it alluded to the possibility of measuring the relative sensitiveness of a photographic plate to the different parts of the spectrum. The plan there indicated, with some instrumental improvements, has been employed in the present instance.

The method employed consists in throwing an image of the photographed spectrum on a white screen and measuring the density of the photograph at different points. As the spectrum of sun light abounds in dark Fraunhofer lines, it was evident that the sun would be a very inconvenient source of light by which to form the spectrum. It was also inconvenient on account of the variation in intensity at different times of the year and day in its different parts. After trials of various sources of light we came to the conclusion that the most practical source to employ was the light from gas, burnt in an Argand burner. A somewhat whiter light would have been better, perhaps, since the ultra-violet rays would have been stronger; but it appeared that, taking all things into consideration, the convenience of gas light more than counterbalanced its disadvantages. We may mention that the crater of the positive pole of the electric light was used in some instances; but, as certain minima of action on some of the salts of silver experimented with lay at parts of the spectrum where bright carbon bands were to be found, the main researches were carried out by the aid of gas light.

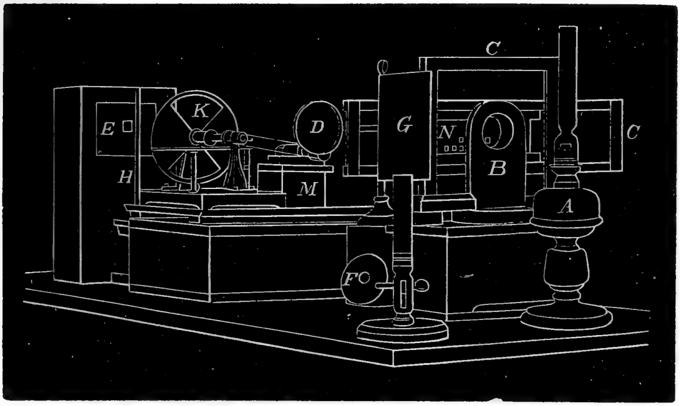
The apparatus employed for photographing the spectrum was that employed in the previous research already alluded to. The two prisms employed were of medium dense white flint, each having an angle of 62°. The collimating lens was of the same material, and the photographic lens was a rapid rectilinear doublet by Dalmeyer, of 16-inch focal length. In some cases one of the lenses of the doublet was dismounted, and the other used as a single lens, giving a focal length of about 30 inches. An image of the gas flame was thrown

on the slit of the collimator, producing a bright image when the slit had a width of $\frac{1}{200}$ of an inch. The bottom half of the slit was closed by a shutter when the gas spectrum was photographed. To know the locality of the part of the gas spectrum impressed, a second spectrum of the electric arc was photographed below the gas spectrum, lithium and sodium being volatilised, to give a sufficient number of fiducial lines. The plate was then withdrawn from the slide and placed in an apparatus by which a series of small square portions of the plate lying parallel and below the last-named spectrum could be exposed at will.

The exposures of this series of squares varied between $3\frac{1}{4}$ seconds and 5 minutes, generally being $3\frac{1}{4}$, $7\frac{1}{2}$, 15, 30, 60, 90, 120, 180, 240, and 300 seconds. The exposure was made to the light from an Argand paraffin lamp placed at 6 feet from the plate. When the height of the flame was kept constant, no practical difference in illumination was found, and no variation was found during a series of exposures if the lamp were allowed to attain a constant temperature by burning ten minutes. The plate thus impressed with the various images was developed in the ordinary manner with the proper developer.

If a gelatine plate were used, ferrous oxalate was usually employed; whilst acidified sulphate of iron was employed if a collodion wet plate were being experimented with. It has not been thought worth while to record all the measurements of the various plates, but a selection has been made of the most important results. It may be remarked that only 5 per cent. addition of bromo-iodide of silver to a bromide of silver emulsion sufficed to shift the place of maximum sensitiveness of the plate from the blue towards the green, as shown in the diagram. After development the plates were fixed as usual and dried, and were then ready for measuring. The following diagram shows the apparatus employed for the measurement, the description of which is taken from a previous paper by one of us.

A is the source of light—gas, paraffin, or other lamp; B is a lens of about 9 inches focus, used as a condenser; C is a double frame for carrying the negative, N, which has an upward and side motion, so that any part of the negative may be brought in front of the condenser; D is a lens on a stand, used to focus the negative on the screen E, which is black except one small square, as shown, where the image of the part to be measured is thrown; F is one of a series of diaphragms used with D for the purpose of sharpening the image and reducing its brightness when required; H is the rod used to cast the shadow on the white patch; G is a flat mirror reflecting a beam also on E; K is the rotating apparatus placed in the path of the light reflected from G, to diminish it at pleasure; M is the small electromotor which drives K. The rod H is so placed that the shadows



cast by the beam from G, and coming through the negative, just touch, and the two are equalised in brightness by means of opening more or less the rotating sectors K.

The negative to be measured was marked with a scale of $\frac{1}{24}$ inches, and, in cases of sudden change of density, to less. It was then placed in the measuring apparatus and measurements commenced. When the square patches were measured, a thickish rod was employed, but for the photographed spectrum a knitting needle, $\frac{1}{8}$ of an inch thick, was substituted. The opacities of the different parts of the film were calculated from where the negative showed "no deposit," and the opening of the sectors when the direct and reflected light balanced was taken as the standard. The required opening of the sectors showed but little variation for any of the photographs.

The scale of density corresponding to different times of exposure was plotted from the readings of squares, and the readings of the different parts of the photographed spectra were applied to the curve so derived, and the density corresponding to the times of exposure tabulated. From the photographed spectrum of the arc the positions of the different measured densities were known and the curve with the reference Fraunhofer lines plotted in the usual manner. From these curves the curves for the normal or wave-length spectrum were calculated, and it is these curves which are shown in the accompanying figures.

Reverting to the paper to which we have referred ('Roy. Soc. Proc.,' No. 217, 1881), it will be seen that the figures therein shown differ from those here given. This is caused by the fact that the former curves were only eye estimations of density, whilst the latter are the comparative sensitivenesses derived from measured densities.

That the former are not in great error will be seen by comparing the place of maximum density of the former with the place of maximum sensitiveness of the latter.

It may be well to remark here on one point to which objection might be taken in the results. It has been assumed in making the scale that length of exposure is equivalent to the intensity of the light. This is not a hasty assumption, but has been carefully tested. When the exposure is but the minute fraction of a second, then the substitution of length of exposure for intensity does not apparently hold good; but, when the exposures are such as are given to the scale, the substitution is perfectly legitimate.

In the following tables the curves of the simple haloid salts of silver are given, as well as mixtures of two or more, and also double salts. Where double salts of silver are shown they were prepared by mixing the alkaline salts in proper equivalent proportions, and then emulsifying in gelatine or collodion by adding the requisite amount of silver nitrate to them. Where simple mixtures are shown, emulsions containing the proper equivalent amount of the silver salts were prepared and subsequently mixed. It has been deemed desirable to give the values for the haloid salts when stained with certain dyes, such as are usually employed in rendering photographic plates what is termed isochromatic. Attention is called to the fact that a mixture of solutions of two dyes does not render the salts of the same sensitiveness to different parts of the spectrum as do the two dyes if applied separately, washing taking place between the application of each. For erythrosin Mallman's well known formula was used to obtain the coloured solution. The erythrosin was obtained from Germany, and showed only traces of fluorescence. The cyanin was obtained from Messrs. Hopkin and Williams, and appears to be made after Greville Williams' formula. When cyanin was employed, 5 grains was dissolved in 1 oz. of alcohol, and water added to make up to 2 ozs. This solution was poured over the plate, which was then allowed to dry. The plate was then washed with equal parts of spirit and water, and finally with water, and then exposed to the spectrum. Similar results were obtained whether the film was dry or moist.

[In all the tables except VII, VIII, IX, X, XV, and XVI, the following are the points on the scale numbers of the principal Fraunhofer lines: H, 13·8; G, 10·9; Li, 9; F, 7·6; E, 6·0; D, 4·3; Li, 2·8.

In Tables VIII and IX the following are to be substituted: H, 14·0; G, 11·0; Li, 9·0; F, 7·7; E, 6·2; D, 4·6; Li, 3·1.

In Tables VII, X, XV, and XVI the following are to be substituted: H, 6·7; G, 9; Li, 10·7; F, 12·1; E, 13; D, 14·3; Li, 15·7. —Feb. 10, 1890.]

Table I.

AgBr. (Collodion plate.) See Fig. 1 (p. 269).

Scale number.	Mean sector reading.	Opacity.	Relative sensitiveness.	Reduced to maximum of 100.	Ordinate of curve to wave-length scale.	Scale. Exposure.		Scale. Sector reading.
						mins.	secs.	
Bare glass	79							
8	75	4	2	1	$0\frac{3}{4}$	5	0	5
$8\frac{1}{2}$	56	23	16	8	..	4	0	$5\frac{1}{2}$
$8\frac{3}{4}$	$34\frac{1}{2}$	$44\frac{1}{2}$	42	21	..	3	0	9
9	25	54	63	32	33	2	0	13
$9\frac{1}{4}$	15	64	104	53	..	1	30	19
$9\frac{1}{2}$	12	67	140	71	..	1	0	26
10	$8\frac{1}{2}$	$70\frac{1}{2}$	188	96	92		30	$43\frac{1}{2}$
$10\frac{1}{2}$	8	71	197	100				
$10\frac{3}{4}$	..	..	..	..	100		15	$59\frac{1}{2}$
11	8	71	197	100	99			
$11\frac{1}{4}$	$8\frac{1}{3}$	71	192	98	97		7	$66\frac{1}{2}$
12	10	69	165	85	87			
$12\frac{1}{2}$	11	68	150	76				
13	$12\frac{1}{2}$	$66\frac{1}{2}$	134	67	68			
$13\frac{1}{2}$	15	64	104	53				
$13\frac{3}{4}$	..	..	..	..	43			
14	20	59	80	41				
$14\frac{1}{4}$	24	55	73	37				
$14\frac{1}{2}$	28	51	55	28				
$14\frac{3}{4}$	$32\frac{1}{2}$	$46\frac{1}{2}$	45	23				
15	37	42	39	20	20			
$15\frac{1}{4}$	42	37	32	16				
$15\frac{1}{2}$	47	32	26	13				
$15\frac{3}{4}$	51	28	21	11				
16	55	24	17	9	9			
$16\frac{1}{2}$	61	18	12	6				
17	65	14	8	4	4			
$17\frac{1}{2}$	70	9	4	2				
18	73	6	3	$1\frac{1}{2}$	$1\frac{1}{2}$			
$18\frac{1}{2}$	$74\frac{1}{2}$	$4\frac{1}{2}$	2	1				
19	76	3	1	$0\frac{1}{2}$	1			

H is 13·8 ; G is 10·9 ; Li is 9.

Table II.

AgCl. (Gelatine plate.) See Fig. 2.

Scale number.	Mean sector reading.	Opacity.	Relative sensitiveness.	Reduced to maximum of 100.	Ordinate of curve to wave-length scale.	Scale.	
						Exposure.	Sector reading.
Bare glass	73½					mins. secs.	
10½	..	..	..	..	1	15	72¾
11	73½	..	..	..	6	30	66¼
11½	72	1½	17	20	19	1 0	43⅓
12	66	7½	30	35	34	1 30	23
12½	60½	13	37½	44	43	2 0	11
13	54½	19	45	53	52		
13¼	51	22½	50	59	58		
13½	44½	29	58½	69	69		
13¾	39	33½	66	78	78		
14	33	40½	75	88	88		
14¼	29	44½	81	95	95		
14½	28	44½	82½	97	97		
14¾	26	46½	85½	100	100		
15	29	43½	81	95	95		
15¼	33	39½	75	88	88		
15½	39	33½	66	78	79		
15¾	45½	28	57	67	67		
16¼	48	24½	54	64	65		
16	53	19½	47	55	57		
16½	56	16½	43	50	51		
17	59½	14	39	46	47		
17½	67	5½	28	33	34		
18	72	1½	17	20	20		
18½	73½	..	..	..	10		
19	..	..	..	..	5		

Table III.

Ag_2BrI. (Gelatine plate.) See Fig. 3.

Scale number.	Mean sector reading.	Opacity.	Relative sensitiveness.	Reduced to maximum of 100.	Ordinate of curve to wave-length scale.	Scale. Exposure.		Scale. Sector reading.
						mins.	secs.	
Bare glass	75							
13	75	0	0	..	..	6	0	17
13½	69	6	3	3½	3½	5	0	15
14	58	17	7	8	8	4	0	16
14½	44	31	16	19	19	3	0	18
15	35½	39½	28	33	33	2	0	19
15½	28	47	48	57	57	1½	0	21
16	24	51	64	76	76	1	0	25
16½	23	52	71	84	84		30	35
17	22	55	77	91	91		15	46
18	21	54	85	100	100		10	51
19	21	54	85	100	100		5	64½
20	22	53	77	91	91			
21	25	50	60	71	71			
22	28	47	48	57	57			
23	30½	44½	40	47	47			
24	34	41	31	37	37			
25	38	37	24	28	28			
26	42	33	18	21	21			
27	47	28	13	15	15			
28	52	23	10	12	12			
29	60	15	6	7	7			
29½	62	13	5½	6½	6½			
30	65½	9½	4	4¾	4¾			
30½	67	8	3½	4	4			
31	70	5	2	2½	2½			
31½	71	4	1½	1¾	1¾			
32	72½	2	1	1	1			

Table IV.

$Ag_2Br.Cl$. (Gelatine plate.) See Fig. 4.

Scale number.	Mean sector reading.	Opacity.	Relative sensitiveness.	Reduced to maximum of 100.	Ordinate of curve to wave-length scale.	Scale.	
						Exposure.	Sector reading.
						mins.	
Bare glass	72						
9	71	1	28	23	23	5	17
10	63	9	49	41	41	4	25
10½	44	28	84	70	70	3	41
11	29	43	112	94	94	2	57
11½	25	47	120	100	100	1½	65
12	29	43	112	94	95	1	71
12½	30	42	110	91	91		
13	37	35	97	81	81		
13½	47	25	79	66	66		
14	58	14	58	48	48		
14½	68	4	38	32	32		
15	71	1	28	23	23		

Table V.

$\left.\begin{array}{l}\frac{3}{4}\ AgBr\\ \frac{1}{4}\ AgI\end{array}\right\}$ Gelatine plate. See Fig. 5.

Scale number.	Mean sector reading.	Opacity.	Relative sensitiveness.	Reduced to maximum of 100.	Ordinate of curve to wave-length scale.	Scale.	
						Exposure.	Sector reading.
						mins. secs.	
Bare glass	75						
3	73	2	3	2	..	4 0	9
3½	66½	8½	6½	4	0·75	3 0	9
4	63	12	9½	6	5·5	2 0	12
5	55	20	15½	10	9	1 35	14½
6	52	23	17½	12	11	1 0	22
6½	46	29	22½	15	..	30	37½
7	44	31	24	16	15	15	56
7½	38	37	29½	17	16	7	66
7¾	35	40	33	..	..	3	73
8	29	45	42½	26½	27	1⅔	71½
8¼	22	53	60	42½			
8½	18	57	75	53½	54	0	75
9	13	62	108	77½			
9½	10½	64½	140	94			
10	10	65	150	100	100		
10½	11	63½	132	88	95		
10¾	13½	61½	104	70	71		
11	14½	60½	95	76	63		
11½	17	58	80	50	54		
12	18	57	75	50	51		
12½	18⅔	56⅓	67	46			
13	20½	54½	65	43	44		
13½	22½	52½	58	39	40		
14	28½	46½	43	29	31		
14½	34	41	34	22½			
15	43	32	25	16½	19		
15½	52	23	17½	12			
16	56½	18½	14	9	9		
17	69	6	5	3½	3		
18	70	5	4½	3	3		
19	74	1	1	⅔	1		

Table VI.

$\left.\begin{matrix}\frac{1}{2}\,AgBr\\ \frac{1}{2}\,AgI\end{matrix}\right\}$ Gelatine plate. See Fig. 6.

Scale number.	Mean sector reading.	Opacity.	Relative sensitiveness.	Reduced to maximum of 100.	Ordinate of curve to wave-length scale.	Scale. Exposure.		Scale. Sector reading.
						mins.	secs.	
Bare glass	79							
2	78	1	½	¾	..	4	0	9½
3	73½	5½	1½	2	2	3	0	11
3½	67	12	4	5½	..	2	0	12
4	63	16	5	7	6	1	30	15
4½	60½	18½	6	8½	..	1	0	18½
5	56	23	7½	10½	9		30	29
5½	53½	25½	8	11	..		15	42
6	50	29	9½	13	12		7	59
6½	49½	29½	9½	13	..		5	63½
7	50	29	9½	13	13		3	70
7½	47½	31½	11	15	15		0	79
8	38	41	17½	24½	23			
8¼	29	50	30	41				
8½	25	54	37½	52	51			
9	18½	50½	62	87	86			
9½	17	62	72	100	100			
10	17½	61½	69	96	96			
10½	20½	58½	52	72	73			
11	23	56	37	51	56			
11½	26	53	36	50				
12	27	52	33½	46½	48			
12½	28	51	32	44½				
13	31	48	26	36	38			
13½	33	46	23	32				
14	39	40	16½	23	24			
14½	44	35	13	18				
15	52	27	8½	12	13½			
15½	59	20	6½	9				
16	64	15	5	7	7			
16½	68½	10½	3½	5				
17	70	9	3	4	4			
18	72½	6½	2	2¾	2½			
19	76	3	1	1½	1½			

Table VII.

$\left.\begin{array}{l}\frac{3}{4}\ AgCl \\ \frac{1}{4}\ AgI\end{array}\right\}$ Gelatine plate. See Fig. 7.

Scale number.	Mean sector reading.	Opacity.	Relative sensitiveness.	Reduced to maximum of 100.	Ordinate of curve to wave-length scale.	Scale.	
						Exposure.	Sector reading.
						mins. secs.	
Bare glass	77½						
4	72½	5	25	10	..	4 0	18
4½	69	7½	38	15	..	3 0	26
5	68	9½	41	16	..	2½ 0	35
6	59½	18	67	26	..	2 0	40
7	52	25½	88	34	..	1½ 0	52
8	46	31½	104	40	..	1 0	66
9	43	34½	112	43	..	30	69
9½	38½	39½	127	49	..	15	76
10	34	43½	145	56	..	0	77½
10¼	32	45½	155	60			
10½	29	48½	170	65			
10¾	40½	37	118	70			
11	54	23½	83	32			
11½	62	15½	60	23			
12	60	17½	66	25			
12½	51½	26	90	35			
13	42	35½	115	44			
13½	33	44½	150	58			
14	23½	54	199	77			
14½	17	60½	248	95			
15	16	61½	260	100			
15½	19	58½	232	89			
16	22½	55½	212	82			
16½	24	53½	195	75			
17	28	49½	175	68			
17½	37	40½	133	52			
18	42	35½	116	45			
18½	56½	21	76	29			
19	65	12½	51	20			
20	74½	3	17	7			

Table VIII.

$\left.\begin{array}{l}\frac{3}{4}AgCl\\ \frac{1}{4}AgBr\end{array}\right\}$ Gelatine plate. See Fig. 8.

Scale number.	Mean sector reading.	Opacity.	Relative sensitiveness.	Reduced to maximum of 100.	Ordinate of curve to wave-length scale.	Scale.	
						Exposure.	Sector reading.
Bare glass	75					sec.	
8	69½	5½	17	9	8	380	9
8½	47	28	45	23	22	300	10½
8¾	36	39	60	31	30	240	12
9	20	55	94	49	48	180	22
9½	12½	62½	118	61	61	120	36
10	9	66	190	99	99	90	46½
10¼	..	..	194	100	100	60	60
10	9	66	190	99	100	30	70
11	10	65	165	85	86	15	75½
12	11½	63½	126	65	69	0	76
13	12	63	120	62	64		
14	17	58	102	53	55		
14¼	19	56	97	50	52		
14½	24	51	85	44	46		
14¾	27	48	78	40	41		
15	35	40	62	32	33		
15¼	39	36	55	29	30		
15½	47	28	45	23	24		
15¾	51	24	40	21	22		
16	57	18	33	17	18		
16½	64	11	25	13	14		
17	65½	9½	23	12	13		
17½	69	6	18	9	10		
18	71	4	15	8	9		

Table IX.

$\left.\begin{array}{l}\frac{3}{4}\ \text{AgBr}\\ \frac{1}{4}\ \text{AgCl}\end{array}\right\}$ Gelatine plate. See Fig. 9.

Scale number.	Mean sector reading.	Opacity.	Relative sensitiveness.	Reduced to maximum of 100.	Ordinate of curve to wave-length scale.	Scalé.		
						Exposure.		Sector reading.
Bare						mins.	secs.	
glass	74							
8	73	1	7	6	6	6	0	5
8¼	72	2	8½	7	7	5	0	6
8½	56½	17½	18	14	14	4	0	7
8¾	43	31	23	19	19	3	0	8
9	18	56	43	35	34	2	0	11
9½	9	65	71	58	58	1½	0	17
10	7	67	120	99	99	1	0	30
10¼	..	..	122	100	100		30	64½
11	9	65	71	58	60		0	74
11½	8	66	90	74	75			
11¾	9	65	71	58	59			
12	12	62	57	47	48			
12½	11	63	60	49	51			
13	16	58	47	39	40			
13½	20	54	40	33	34			
13¾	24	50	35	29	30			
14	31	43	29	24	25			
14¼	35	39	26	21	21			
14½	46	28	21	17	18			
14¾	51	23	20	16	17			
15	60	14	17	14	14			
15½	66	8	14	11	11			
15¼	69	5	12	10	10			
16	72	2	8	7	7			

Table X.

AgBr stained with Erythrosin. (Gelatine plate.) See Fig. 10.

Scale number.	Mean sector reading.	Opacity.	Relative sensitiveness.	Reduced to maximum of 100.	Ordinate of curve to wave-length scale.	Scale.	
						Exposure.	Sector reading.
Bare glass	110					secs.	
4	100	10	1	1	1	120	107·5
5	80	30	4	4·4	5	100	12
6	59	51	7·5	8·5	10	90	13
7	39	71	17	19	21	80	14·25
8	33	77	23	26	31	60	18
9	27	83	13	14·3	15·5	50	20·5
10	25·5	84·5	37	40·7	43·5	40	24
10½	35	75	30	33	35	30	28
11	53	57	21	23	24·5	20	35
11½	59	51	7·5	8·5	8·5	15	43
12	44	66	14	15·4	16	10	53
12½	25·5	84·5	37	40·7	42	5	69
13	15	95	74	82	83	0	110
13½	13	97	19	100	100		
14	35	75	21	23	23		
14½	86	24	6	6·6	6		
15	108	2	0·5	0·5	0·3		

Table XI.

$\left.\begin{array}{l}\frac{3}{4}\text{ AgBr}\\ \frac{1}{4}\text{ AgI}\end{array}\right\}$ Dyed with Erythrosin. See Fig. 5.

Scale number.	Mean sector reading.	Opacity.	Relative sensitiveness.	Reduced to maximum of 100.	Ordinate of curve to wave-length scale.	Scale.	
						Exposure.	Sector reading.
Bare glass	78					mins. secs.	
4¼	48	30	10	12½	12	2 0	17½
4½	32	46	26	32½	27	1 30	19½
4¾	23½	54½	57	71	..	1 0	24½
5	20	58	82	100	100	30	30
5¼	26	52	45	56	..	15	40½
5½	34½	43	22	27	28	10	48
5¾	47	31	10½	13	..	5	70½
6	55	23	7½	9½	9	0	78
6¼	61	17	6½	8			
6½	64	14	6	7½	7		
7	68	10	5½	7			
8	69½	8½	5	6¼	7		
8¼	64½	13½	6	7½			
8½	57	21	7	8¾			
9	45	33	11½	14¼	15		
9¼	40½	37½	15	18¾			
9½	37	41	19½	24	26		
10	36½	41½	19	23½	25		
10½	40	38	15	18¾			
10¾	44	34	12	15			
11	47½	30½	10	12½	14		
11½	52	26	8½	10½			
12	54½	23½	7½	9½	10½		
12½	57	21	7	8¾			
13	59	19	7	8¾	10		
13¼	63	15	6½	8			
13½	65	13	6	7½			
14	69	9	5½	7	8		
14½	73	5	4	5			
15	76	2	2	2½	3		
15½	76½	1½	1½	2			

Table XII.

$\left.\begin{array}{l}\frac{1}{2}AgBr\\ \frac{1}{2}AgI\end{array}\right\}$ Dyed with Erythrosin. See Fig. 6.

Scale number.	Mean sector reading.	Opacity.	Relative sensitiveness.	Reduced to maximum of 100.	Ordinate of curve to wave-length scale.	Scale. Exposure.		Scale. Sector reading.
						mins.	secs.	
Bare glass	75							
4½	35½	39½	31	21½	18	4	0	25
4¾	29½	45½	77	53	..	3	0	27⅓
5	28	47	145	100	100	2	0	28¾
5¼	34	41	36	25	..	1	30	29
5½	43	32	20	14	14	1	0	30¾
5¾	53	22	12½	8½	..		30	36
6	56	19	11	7½	8		15	48¾
6¼	64	11	8	12	..		10	58½
6½	64	11	8	12	12		5	71
6¾	67	8	6½	4½			0	75
7	71	4	5	3½	3			
7½	71	4	5	3½	3			
8	69	6	6	4	4			
8½	63	12	8½	6				
9	57	18	10½	7	7			
9¼	55	20	11½	8				
9½	53	22½	12½	8½				
9¾	54½	20	11½	8				
10	52	23	13	9	10			
10¼	55	20	11½	8				
10½	58	17	10	7				
10¾	63	12	8½	6				
11	63	12	8½	6	6			
11¼	66	9	7	5				
11½	67	8	6½	4½				
12	68	7	6	4	4			
12½	69	6	6	4				
13	70	5	5	3½	3			
14	73	2	4	2¾	2½			
15	74½	½	1	¾	¾			

Table XIII.

AgCl dyed with Erythrosin. (Gelatine plate.) See Fig. 11.

Scale number.	Mean sector reading.	Opacity.	Relative sensitiveness.	Reduced to maximum of 100.	Ordinate of curve to wave-length scale.	Scale.		
						Exposure.		Sector reading.
						mins.	secs.	
1	250	0·2	6	0·4	0·5			22
2	240	0·45	16	1	1	5	0	93
3	232	0·8	24	2	2	4	0	27½
4	208	1·4	48	3	3·5	3	0	57
5	192	2	64	4·3	4·5	1	0	83
6	176	3	80	6·4	7	½	0	108
7	220	1	36	2·2	2·4	¼	0	132
8	245	0·25	11	0·5	0·4	⅛	0	150
9	256	0	0	0	0		5	176
11	256	0	0	0	0		3	
11½	192	2	64	4·3	4·5			
12	160	4·5	96	10	10			
12½	128	8	28	27	17			
13	84	30	72	65·1	65			
13½	66	46	90	100	100			
14	99	20	157	43·4	43			
14¼	192	2	64	4·3	4			
14½	220	1	36	2·2	2			

Table XIV.

$\left.\begin{array}{l}\frac{3}{4}AgBr\\ \frac{1}{4}AgI\end{array}\right\}$ Dyed with Erythrosin and Cyanin. See Fig. 12.

Scale number.	Mean sector reading.	Opacity.	Relative sensitiveness.	Reduced to maximum of 100.	Ordinate of curve to wave-length scale.	Scale.		
						Exposure.		Sector reading.
						mins.	secs.	
Bare glass	47							
2	43	4	2	9½	9	1	0	8
2½	40	7	3½	16	16	0	30	10⅔
2¾	34½	12½	7	32½	30½	0	15	23½
3	27	20	12½	58	61	0	10	30
3½	17	30	21½	100	100	0	5	37¼
4	23	24	15½	72	73	0	0	47
4¼	23	24	15½	72	74			
4½	23½	23½	15	70	65			
4¾	19	28	19	88	91			
5	20	27	18	84	88			
5¼	30	17	10	46	48			
5½	33	14	8	37	38			
5¾	35	12	6½	30	31			
6	37	10	5	23	25			
6½	36½	10½	5½	25	27			
7	38½	8½	4½	21	23			
7½	40	7	3½	16	18			
8	40	7	3½	16	18			
8½	36	11	6	28	30			
9	32½	14½	8½	39	42			
9½	28	19	11½	53	56			
10	28	19	11½	53	56			
10½	29½	17½	10	46	48			
11	32	15	8½	39	42			
11½	34	13	7	32	30			
12	34	13	7	32	30			
13	34	13	7	32	17			

Table XV.

Edwards' Isochromatic Plate. See Fig. 13.

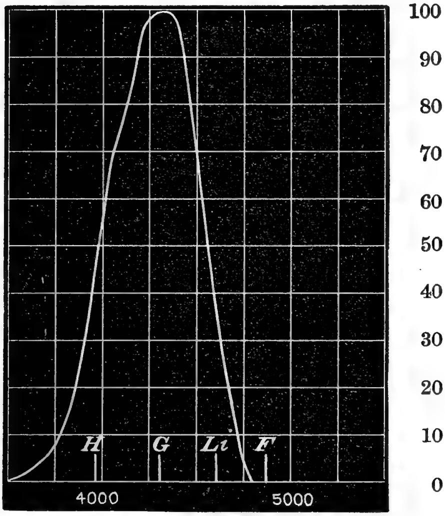
Scale number.	Mean sector reading.	Opacity.	Relative sensitiveness.	Reduced to maximum of 100.	Ordinate of curve to wave-length scale.	Scale.	
						Exposure.	Sector reading.
						secs.	
5	130	8	2	1·7	2	180	16
6	128	10	3	2·5	2·5	120	22
7	120	18	5	4·4	4·5	100	29
7½	112	26	7	6	6	80	42
8	106	32	9	7·8	8·5	60	16
8½	100	38	11	9·6	10	50	22
9	75	63	19	16·6	18	44	30
9½	63	75	22·5	19·8	21·5	30	44
10	50	88	27·5	24	25	25	56
10½	46	92	29	25·4	27	20	72
11	50	88	27·5	24	25	15	88
11¼	66	72	22	19·2	20	10	104
11½	78	60	18	15·8	16	5	120
12	90	48	14·5	12·6	12·5	0	138
12¼	74	64	19·5	17	17		
12½	53	85	26·5	23	23·5		
12¾	26½	111½	44	39·5	40		
13	17½	120½	57	50	50		
13¼	6	132	102	91	92		
13½	5¾	132¼	115	100	100		
13¾	6½	131½	106	92·5	93		
14	10	128	83	72·5	71		
14¼	63	71	22·5	19·6	19		
14½	120	18	5	4·7	4		
15	138						

Table XVI.

Edwards' Isochromatic Plate treated with Cyanin. See Fig. 14.

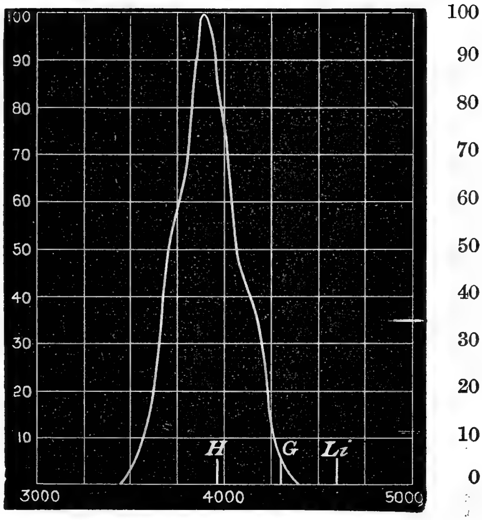
Scale number.	Mean sector reading.	Opacity.	Relative sensitiveness.	Reduced to maximum of 100.	Ordinate of curve to wave-length scale.	Scale.	
						Exposure.	Sector reading.
						secs.	
5	96	10	2	3·5	4	90	3
6	92	14	2·5	4	4·1	80	4
7	79	27	5	8	8·5	60	6
8	64	42	8·5	11	12	50	7·5
9	42	64	14	22	25	40	10
9½	32	74	18	29	17·5	30	16
10	23	83	23·5	37	41	20	28
11	19	87	26·5	42·5	47	15	40
11¼	23	83	23·5	37·5	41	10	57
11½	29	77	19·5	31	33	5	79
11¾	36·5	69·5	16	26	28	0	106
12	39	67	15·5	25	26·5		
12½	33·5	72·5	17	27·5	29		
12¾	26·5	79·5	21	33·5	36		
13	18	88	27·5	44·5	46		
13¼	11	95	38	61·5	64		
13½	6¾	99¼	55	88	92		
13¾	7	99	53	84·5	89		
14	9	97	44	69·5	72		
14¼	9	97	44	69·5	70·5		
14½	6¼	99¾	58	93	94·5		
14¾	5½	100½	63	100	100		
15	10	96	40	63	63		
15¼	24	82	23	37·5	37		
15½	41	65	14·5	22·5	22		
15¾	71	35	7	11·5	11		
16	86	20	3	5	4·5		

Fig. 1.



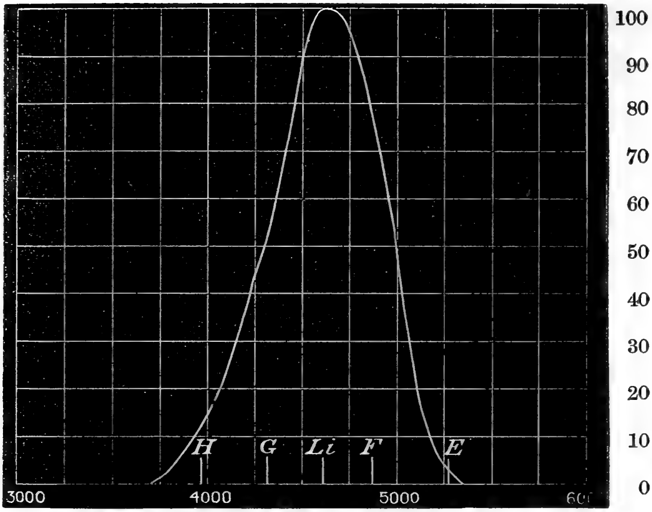
AgBr.

Fig. 2.



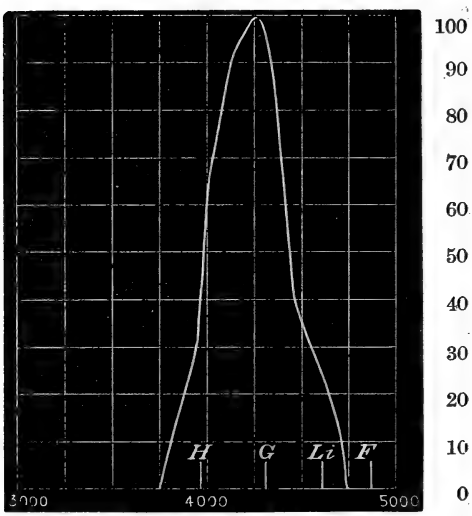
AgCl.

Fig. 3.



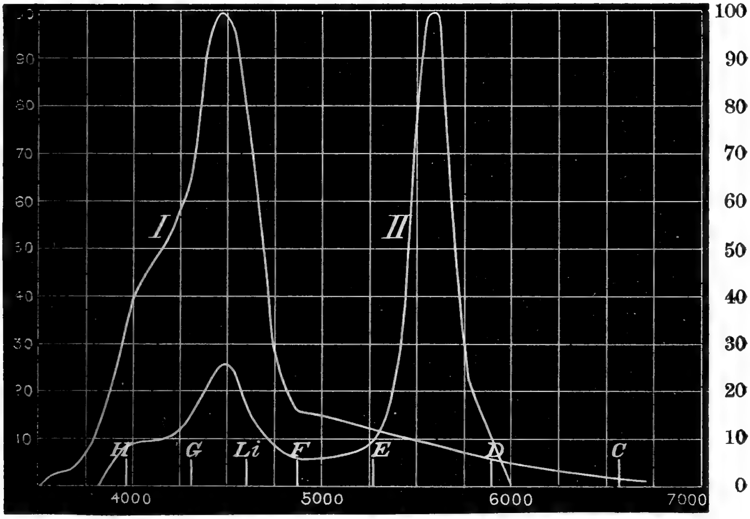
Ag_2BrI.

Fig. 4.



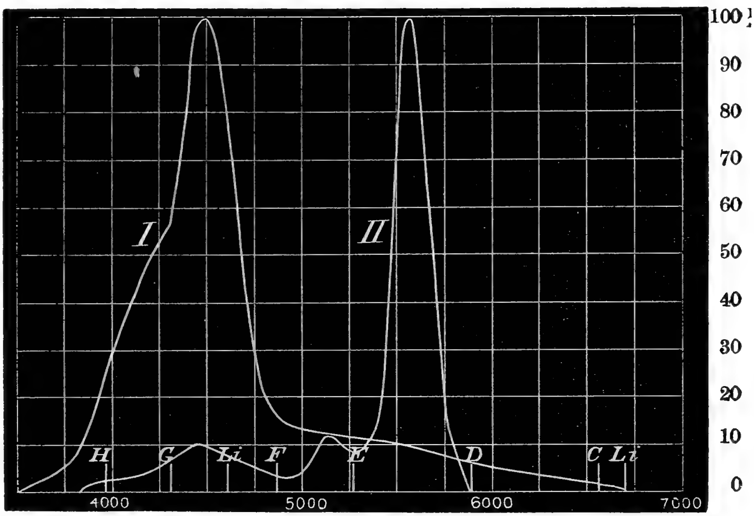
Chloro-bromide.

FIG. 5.



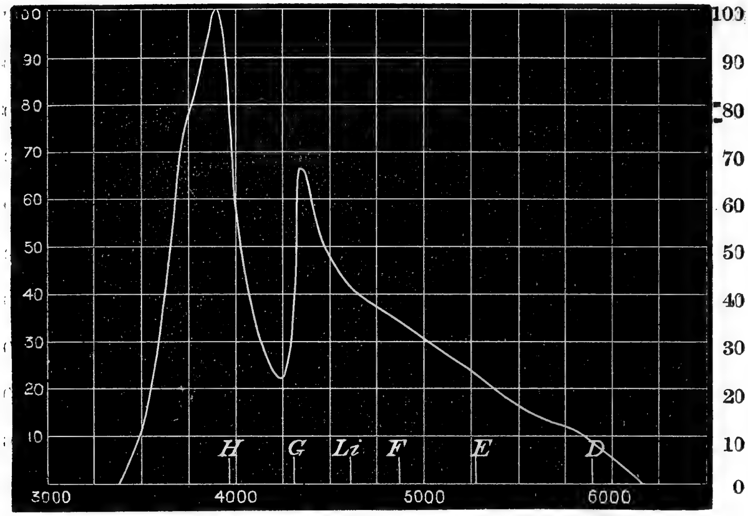
I is $\left.\begin{matrix}\frac{3}{4}AgBr\\ \frac{1}{4}AgI\end{matrix}\right\}$ II is $\left.\begin{matrix}\frac{3}{4}AgBr\\ \frac{1}{4}AgI\end{matrix}\right\}$ Dyed with erythrosin.

FIG. 6.



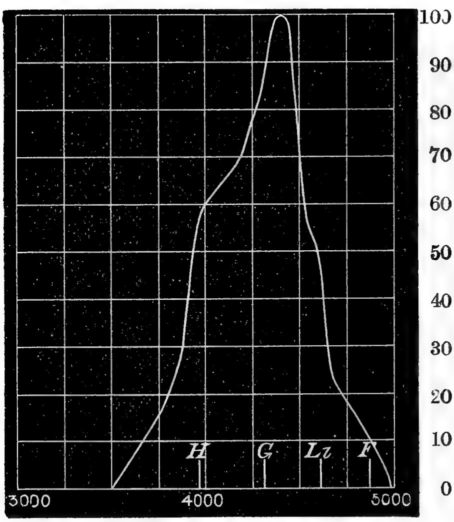
I is $\left.\begin{matrix}\frac{1}{2}AgBr\\ \frac{1}{2}AgI\end{matrix}\right\}$ II is $\left.\begin{matrix}\frac{1}{2}AgBr\\ \frac{1}{2}AgI\end{matrix}\right\}$ Stained with erythrosin.

FIG. 7.



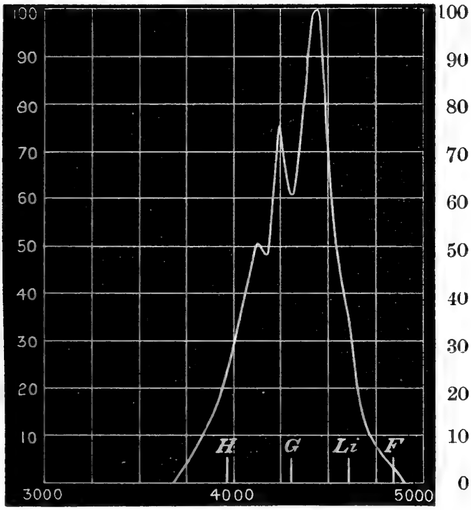
$\left.\begin{matrix}\frac{3}{4}AgCl \\ \frac{1}{4}AgI\end{matrix}\right\}$

FIG. 8.



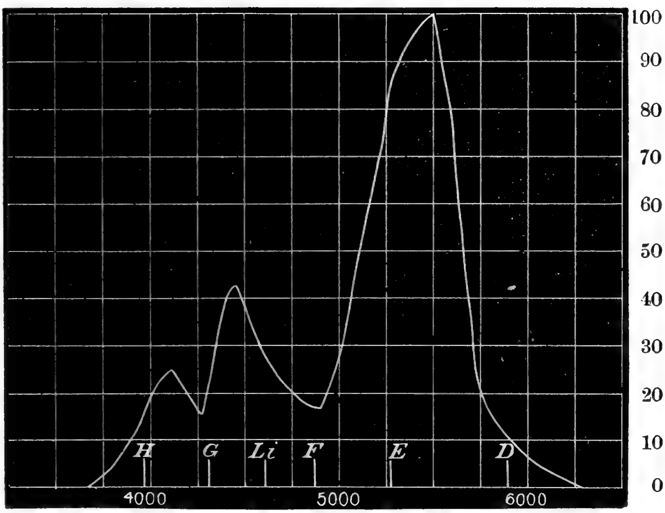
$\left.\begin{matrix}\frac{3}{4}AgCl \\ \frac{1}{4}AgBr\end{matrix}\right\}$

FIG. 9.



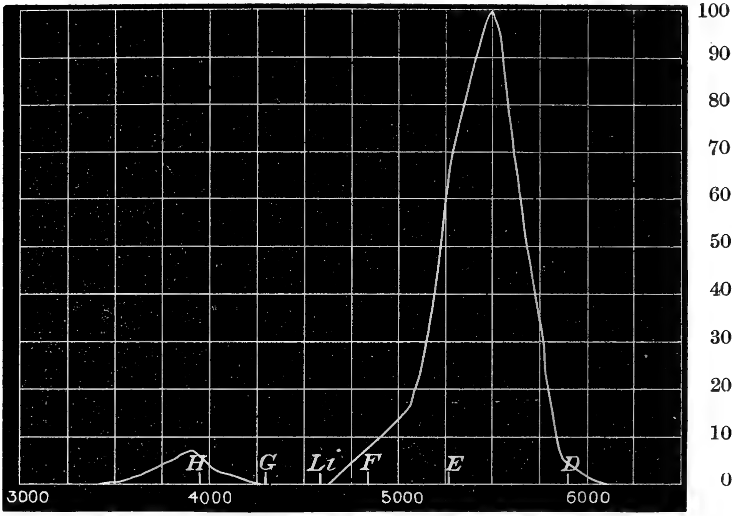
$\frac{3}{4}$AgBr
$\frac{1}{4}$AgCl

FIG. 10.



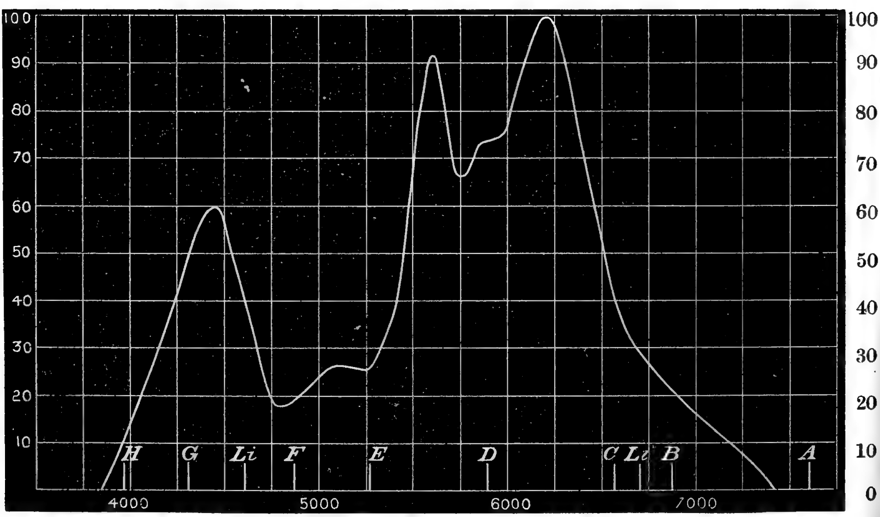
AgBr stained with erythrosin.

Fig. 11.



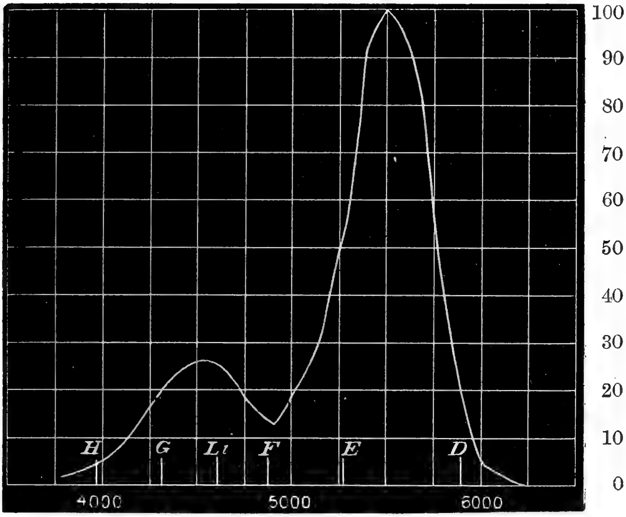
AgCl stained with erythrosin.

Fig. 12.



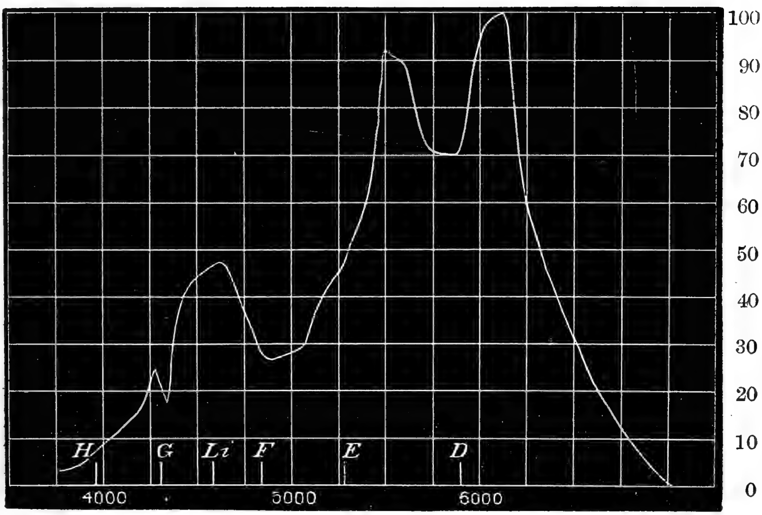
$\left.\begin{array}{l}\frac{3}{4}\,AgBr\\ \frac{1}{4}\,AgI\end{array}\right\}$ Stained with erythrosin and cyanin.

FIG. 13.



Edwards' isochromatic.

FIG. 14.



Edwards' isochromatic treated with cyanin.

March 6, 1890.

Sir G. GABRIEL STOKES, Bart., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of the Candidates for election into the Society were read from the Chair, as follows:—

Baker, Sir Benjamin, M.Inst.C.E.
Bateson, William, M.A.
Bosanquet, Robert Holford Macdowall, M.A.
Burbury, Samuel Hawkesley, M.A.
Buzzard, Thomas, M.D.
Cameron, Sir Charles Alexander, M.D.
Carnelley, Professor Thomas, D.Sc.
Clark, John Willis, M.A.
Conroy, Sir John, Bart., M.A.
Corfield, William Henry, M.D.
Crisp, Frank, LL.B.
Cunningham, Professor Daniel John, M.D.
Davis, James William, F.G.S.
Dawson, George Mercer, D.Sc.
Dresser, Henry Eales, F.L.S.
Eaton, Rev. Alfred Edwin, M.A.
Edgeworth, Professor Francis Ysidro, M.A.
Elgar, Professor Francis, LL.D.
Elliott, Edwin Bailey, M.A.
Ellis, William, F.R.A.S.
Ewart, Professor J. Cossar, M.D.
Frankland, Professor Percy Faraday, B.Sc.
Gadow, Hans, M.A.
Gardiner, Walter, M.A.
Giffen, Robert, LL.D.
Gilchrist, Percy C.
Gotch, Francis, M.R.C.S.
Halliburton, William Dobinson, M.D.
Harker, Alfred, M.A.
Heath, Christopher, F.R.C.S.
Herdman, Professor William Abbott, D.Sc.
Hickson, Sydney John, D.Sc.
Hinde, George Jennings, Ph.D.
Howorth, Henry Hoyle.
Kerr, John, LL.D.
King, George.
Lansdell, Rev. Henry, D.D.
Lea, Arthur Sheridan, D.Sc.
Macalister, Donald, M.D.
MacMahon, Percy Alexander, Major R.A.
MacMunn, Charles, M.D.
Marr, John Edward, M.A.
Martin, John Biddulph, M.A.
Matthey, Edward, F.C.S.
Miall, Professor Louis C.
Mond, Ludwig, F.C.S.
Nicholson, Professor Henry Alleyn, M.D.
Norman, Rev. Alfred Merle, M.A.
Ord, William Miller, M.D.
Palmer, Henry Spencer, Major-General R.E.
Pedler, Professor Alexander, F.C.S.
Perkin, Professor William Henry, jun., F.C.S.
Pickering, Professor Spencer Umfreville, M.A.
Roberts, Isaac, F.R.A.S.
Ross, James, M.D.

Rutley, Frank, F.G.S.
Sankey, Matthew Henry P. R., Capt. R.E.
Saunders, Howard, F.L.S.
Scott, Alexander de Courcy, Major-General R.E.
Seebohm, Henry, F.L.S.
Sharp, David, M.B.
Shaw, William Napier, M.A.
Smith, Willoughby.
Stebbing, Rev. Thomas Roscoe Rede, M.A.
Stevenson, Thomas, M.D.
Sutton, J. Bland, F.R.C.S.
Teall, J. J. Harris, M.A.
Thompson, Professor Silvanus Phillips, D.Sc.
Thorne, Richard Thorne, M.D.
Thornycroft, John Isaac, M. Inst. C.E.
Tizard, Thomas Henry, Staff-Commander R.N.
Veley, Victor Hugo, M.A.
Waller, Augustus D., M.D.
Weldon, Walter Frank Raphael, M.A.
Whitehead, Charles, F.L.S.

The following Papers were read :—

I. "On a second Case of the Occurrence of Silver in Volcanic Dust, namely, in that thrown out in the Eruption of Tunguragua in the Andes of Ecuador, January 11th, 1886." By J. W. MALLET, F.R.S., V.P.C.S., University of Virginia. Received February 7, 1890.

In a paper laid before the Royal Society three years ago,* I gave an account of a specimen of volcanic dust, or so-called ash, from the eruption of Cotopaxi of July 22nd and 23rd, 1885, in which ash silver was found to be present in minute quantity, that being the first instance in which this metal had been detected among the materials ejected from volcanoes. The specimen was sent me by my friend and former pupil Señor Julio R. Santos, of Bahia de Caraguez, Ecuador.

In his letter accompanying it, dated March 8th, 1886, he said, "On the 11th and 12th of January there was a terrific eruption from Mount Tunguragua. It is more than a century ago since the last eruption of this volcano." I requested Señor Santos to procure for me if possible some of the ash from this unexpected outburst of Tunguragua, which he kindly promised to do, and at once took steps for the purpose, but his own absence from home and protracted stay at Panama delayed the matter, and only in the early part of the present year (1889) did the specimen reach me.

In sending it, on the 8th of February last, he wrote to me that "it was collected in Guayaquil on the 11th of January, 1886, on sheets of clean cloth, by Señor Ancisar Montalvo . . . Mount Tunguragua had been silent for over a century; on the 11th of

* See 'Roy. Soc. Proc.,' vol. 42, 1887, p. 1.

January, 1886, began its eruption, and it continued in eruption till November of the same year."

The volcano in question—about 16,500 feet in height—is one of the great mountains of the Eastern Cordillera of Ecuador, lying some 50 or 60 miles south and a little east of Cotopaxi, in between 1° and 2° of south latitude, and about 85 or 90 miles in a direct line from Guayaquil on the coast of the Pacific, where the specimen of ash was collected. The appearance of the regular cone-shaped summit, rising directly from a plain of only 5700 feet above sea level, is described as very striking, crowned with perpetual snow, and resembling in aspect Cotopaxi. Villavicencio in his work on Ecuador thus speaks of it—"*Esta bellísima montaña está collocada á un nivel inferior de todas las otras montañas del Ecuador, la que hace que su altura aparente, mirada desde su base, sea mucho mayor que la del Chimborazo. . . . Este volcan tiene la figura de un cono perfecto, cuya parte superior está cubierta de nieves sempiternas que forman una especie de capucha la que, contrastando con las negras peñolerias de su base i algunos bosques, le dan un aspecto sublime i bello: su descenso es rápido por todas partes, escepto por el lado en que se une con la cordillera, por cuya parte se puede subir hasta principiar las nieves; pero de allí en adelante hay obstáculos para el viajero mas intrépido. La nieve deshecha de su copa se precipita en cascadas elevadísimas, que aumentan la hermosura de ese sublime cono, que tiene de notable el encontrarse en él todos los climas, desde el frio de la Siberia, que comienza en el límite de sus nieves, hasta el de 27° del centígrado que tiene su base.*"

The most notable eruption recorded was that of 1777. The mountain was long classed with *apagados* or extinct volcanoes, although from time to time during the last century several explorers have reported "smoke" as seen issuing from the summit. It is generally believed in the surrounding country that a subterranean communication, following the line of the Andes, exists between this volcano and Cotopaxi, the one showing signs of activity when the other is in eruption, and becoming quiet when such eruption ceases.

The specimen of Tunguragua volcanic ash received by me had very much the same appearance as that of the Cotopaxi ash formerly examined—a very finely divided powder, of light brownish-grey or fawn colour, a little lighter in shade than the Cotopaxi specimen. Under the microscope the same minute grains and spiculæ were seen, having for the most part sharp, splintery edges and angles. The same general mineralogical character was observed—essentially the *débris* of trachytic "andesite"—with apparently less difference in the colour of the felspar present, which was almost all white or colourless, and with fewer disseminated particles of magnetite and specular iron. There were some scales of what was probably comminuted vesicular pumice.

The specific gravity was found = 2·597 at 25° C., as compared with water at the same temperature.

The ash was fusible, though with difficulty, before the mouth blow-pipe, or in a small platinum spoon or crucible over the blast lamp, forming a greenish vesicular slag, red on the surface.

On being boiled with water it gave up 0·13 per cent. of soluble matter. This portion, soluble in water, consisted mainly of sodium sulphate and chloride, with a little of the corresponding salts of potassium and traces of yellowish-brown organic matter.

Treatment with dilute hydrochloric or even acetic acid produced quite noticeable effervescence. On digestion with gentle warming in rather dilute acetic acid, 3·36 per cent. dissolved, of which 2·83 per cent. was calcium carbonate, 0·31 per cent. magnesium carbonate, and the rest minute amounts of iron, aluminum, and the alkaline salts soluble in water.

On being boiled with strong hydrochloric acid the ash (not having been previously treated with water or acetic acid) dissolved to the extent of 9·61 per cent., the solution having a dark yellow-brown colour, due to the presence of iron.

The material taken as a whole, *i.e.*, without any previous mechanical separation of its constituent minerals, and without previous digestion with water or acid, but dried at 100° C., gave on analysis the following results* :—

SiO_2	61·49	
TiO_2	0·18	
Al_2O_3	16·05	
Fe_2O_3	2·84	
FeO	2·48	
MnO	trace	
MgO	1·04	besides the $MgCO_3$ and
CaO	3·39	$CaCO_3$ stated below.
Na_2O	6·85	
K_2O	2·14	
Li_2O	trace	
Ag	"	
Cl	"	
SO_4	"	
Organic matter	"	
$CaCO_3$	2·83	
$MgCO_3$	0·31	
H_2O	0·27	
	99·87	

* For comparison with these results may be quoted the following :—

A. Abich's analysis of the rock, andesite, from the summit of Chimborazo. (G.

On finding the carbonates of calcium and magnesium, I was at first inclined to suspect that they were merely impurities, which had become mingled with the ash in collecting it—fragments of plaster from a wall, or something of that kind—but from their pretty uniform distribution, which was ascertained by two or three separate experiments, and from their fine state of division, no particles separately detectable by the naked eye being found, this does not seem likely, and it is rather to be supposed that these carbonates are present as minute particles of a magnesian limestone, torn away by the larger ejected masses from some portion of the inner surface of the crater of the volcano. A large part of the silver of South America is said to occur associated with limestones of Carboniferous age in the Bolivian and Peruvian Andes, but whether such rocks have been observed to extend into Ecuador I do not know.

As for the distinct trace of organic matter observed, this may in part represent ordinary dust swept down from the lower regions of the atmosphere, but was no doubt partly at least derived from the cloth on which the volcanic ash is said to have been collected, since a few cotton fibres were easily identified under the microscope.

It was proved, as in the case of the ash from Cotopaxi formerly examined, that the silver present in minute quantity could be dissolved out by boiling with an aqueous solution of ammonia, or of potassium cyanide, or of sodium thiosulphate, but was not appreciably extracted by nitric acid. Hence, as was remarked in the former paper, it seems

Bischof, 'Elements of Chem. and Phys. Geology, translated for the Cavendish Society,' vol. 3, pp. 392—393.)

B. 1, 2, and 3. Analyses of dust from the great eruption of Krakatoa in the Sunda Strait, August 26 and 27, 1883. ('Report of the Committee of the Royal Society on the Eruption of Krakatoa,' p. 40.) 1, referring to the dust which fell at Krakatoa; 2, to that which fell at points within 100 miles from the volcano; and 3, to a specimen which fell nearly 900 miles from the volcano; volatile matters are omitted, and the totals calculated to 100 parts.

	A.	B. 1.	B. 2.	B. 3.
SiO_2	65·09	61·36	66·77	68·99
TiO_2	—	1·12	0·67	0·39
Al_2O_3	15·58	17·77	16·44	15·24
Fe_2O_3	3·83	4·39	3·41	0·28
FeO	1·73	1·71	1·37	3·72
MnO	—	0·41	0·38	trace
MgO	4·10	2·32	1·67	0·83
CaO	2·61	3·45	2·90	2·76
Na_2O	4·46	4·98	4·14	4·32
K_2O	1·99	2·51	2·25	3·47
Loss by ignition	0·41	—	—	—
	99·8	100·02	100·00	100·00

most probable that the metal exists in the ash as silver chloride. No other heavy metal than iron and silver could be detected. The most scrupulous care was taken in proving that the silver did not come from any of the vessels or reagents employed.

By carefully made assays, both in the dry and liquid way, it was found that the silver was present to the extent of about 1 part in 107,200 of the ash, or between a fourth and a third of a Troy ounce to the ton of 2240 pounds. This is rather a smaller proportion than was found in the Cotopaxi ash, which contained about 1 part for 83,600, or two-fifths of an ounce per ton. In both eruptions the fall of considerable quantities of ashes at points on the sea coast so distant from the respective volcanoes as Guayaquil and Bahia de Caraguez indicates that in the aggregate very large absolute amounts of silver must have been ejected and dispersed.

These two appear to be the only cases in which silver has been detected among the materials thrown out by volcanoes. Although the fact is well known that the chain of the Andes has for centuries yielded this metal in great abundance, it is worthy of notice that, to the depths as yet reached by mining work, Ecuador is less rich in valuable minerals, and especially the precious metals, than any other of the South American States.

II. "On the Tension of Recently Formed Liquid Surfaces." By Lord Rayleigh, Sec. R.S. Received February 13, 1890.

It has long been a mystery why a few liquids, such as solutions of soap and saponine, should stand so far in advance of others in regard to their capability of extension into large and tolerably durable laminæ. The subject was specially considered by Plateau in his valuable researches, but with results which cannot be regarded as wholly satisfactory. In his view the question is one of the ratio between capillary tension and superficial viscosity. Some of the facts adduced certainly favour a connexion between the phenomena attributed to the latter property and capability of extension; but the "superficial viscosity" is not clearly defined, and itself stands in need of explanation.

It appears to me that there is much to be said in favour of the suggestion of Marangoni* to the effect that both capability of extension and so-called superficial viscosity are due to the presence upon the body of the liquid of a coating or pellicle composed of matter whose inherent capillary force is less than that of the mass. By

* 'Nuovo Cimento,' vol. 5–6, 1871–72, p. 239.

means of variations in this coating, Marangoni explains the indisputable fact that in vertical soap films the effective tension is different at various levels. Were the tension rigorously constant, as it is sometimes inadvertently stated to be, gravity would inevitably assert itself, and the central parts would fall 16 feet in the first second of time. By a self-acting adjustment the coating will everywhere assume such thickness as to afford the necessary tension, and thus any part of the film, considered without distinction of its various layers, is in equilibrium. There is nothing, however, to prevent the interior layers of a moderately thick film from draining down. But this motion, taking place as it were between two fixed walls, is comparatively slow, being much impeded by ordinary fluid viscosity.

In the case of soap, the formation of the pellicle is attributed by Marangoni to the action of atmospheric carbonic acid, liberating the fatty acid from its combination with alkali. On the other hand, Sondhauss* found that the properties of the liquid, and the films themselves, are better conserved when the atmosphere is excluded by hydrogen; and I have myself observed a rapid deterioration of very dilute solutions of oleate of soda when exposed to the air. In this case a remedy may be found in the addition of caustic potash. It is to be observed, moreover, that, as has long been known, the capillary forces are themselves quite capable of overcoming weak chemical affinities, and will operate in the direction required.

A strong argument in favour of Marangoni's theory is afforded by his observation,† that within very wide limits the superficial tension of soap solutions, as determined by capillary tubes, is almost independent of the strength. My purpose in this note is to put forward some new facts tending strongly to the same conclusion.

It occurred to me that, if the low tension of soap solutions as compared with pure water was due to a coating, the formation of this coating would be a matter of time, and that a test might be found in the examination of the properties of the liquid surface immediately after its formation. The experimental problem here suggested may seem difficult or impossible; but it was, in fact, solved some years ago in the course of researches upon the Capillary Phenomena of Jets.‡ A jet of liquid issuing under moderate pressure from an elongated, *e.g.*, elliptical, aperture perforated in a thin plate, assumes a chain-like appearance, the complete period, λ, corresponding to two links of the chain, being the distance travelled over by a given part of the liquid in the time occupied by a complete transverse vibration of the column about its cylindrical configuration of equilibrium. Since the

* 'Poggendorff, Annalen,' Ergänzungsband 8, 1878, p. 266.

† 'Poggendorff, Annalen,' vol. 143, 1871, p. 342. The original pamphlet dates from 1865.

‡ 'Roy. Soc. Proc.,' May 15, 1879.

phase of vibration depends upon the time elapsed, it is always the same at the same point in space, and thus the motion is *steady* in the hydrodynamical sense, and the boundary of the jet is a fixed surface. Measurements of λ under a given head, or velocity, determine the time of vibration, and from this, when the density of the liquid and the diameter of the column are known, follows in its turn the value of the capillary tension (T) to which the vibrations are due. *Cœteris paribus*, $T \propto \lambda^{-2}$; and this relation, which is very easily proved, is all that is needed for our present purpose. If we wish to see whether a moderate addition of soap alters the capillary tension of water, we have only to compare the wave-lengths λ in the two cases, using the same aperture and head. By this method the liquid surface may be tested before it is $\frac{1}{100}$ second old.

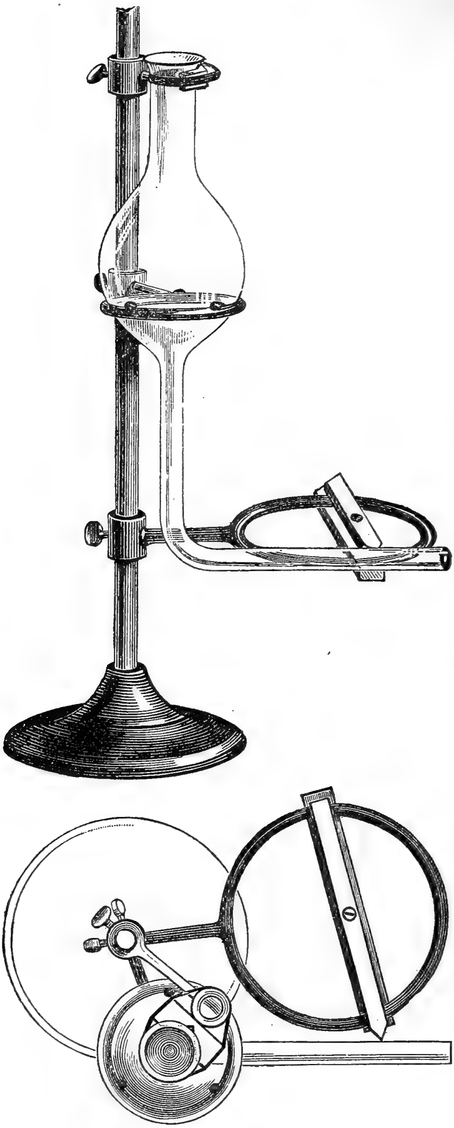
Since it was necessary to be able to work with moderate quantities of liquid, the elliptical aperture had to be rather fine, about 2 mm. by 1 mm. The reservoir was an ordinary flask, 8 cm. in diameter, to which was sealed below as a prolongation a (1 cm.) tube bent at right angles (figs. 1, 2). The aperture was perforated in thin sheet brass, attached to the tube by cement. It was about 15 cm. below the mark, near the middle of the flask, which defined the position of the free surface at the time of observation.

The arrangement for bringing the apparatus to a fixed position, designed upon the principles laid down by Sir W. Thomson, was simple and effective. The body of the flask rested on three protuberances from the ring of a retort stand, while the neck was held by an india-rubber band into a V-groove attached to an upper ring. This provided five contacts. The necessary sixth contact was effected by rotating the apparatus about its vertical axis until the delivery tube bore against a stop situated near its free end. The flask could thus be removed for cleaning without interfering with the comparability of various experiments.

The measurements, which usually embraced two complete periods, could be taken pretty accurately by a pair of compasses with the assistance of a magnifying glass. But the double period was somewhat small (16 mm.), and the little latitude admissible in respect to the time of observation was rather embarrassing. It was thus a great improvement to take magnified photographs of the jet, upon which measurements could afterwards be made at leisure. In some preliminary experiments the image upon the ground glass of the camera was utilised without actual photography. Even thus a decided advantage was realised in comparison with the direct measurements.

Sufficient illumination was afforded by a candle flame situated a few inches behind the jet. This was diffused by the interposition of a piece of ground glass. The lens was a rapid portrait lens of large

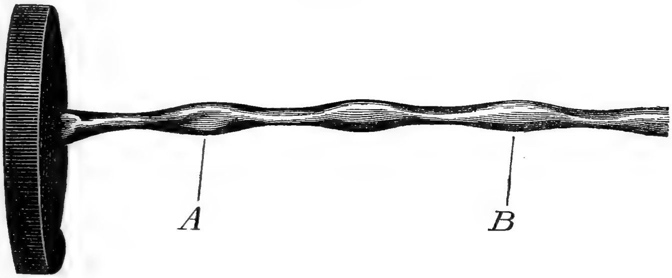
FIGS. 1 and 2.



aperture, and the ten seconds needed to produce a suitable impression upon the gelatine plate was not so long as to entail any important change in the condition of the jet. Otherwise, it would have been easy to reduce the exposure by the introduction of a condenser. In

all cases the sharpness of the resulting photographs is evidence that the sixth contact was properly made, and thus that the scale of magnification was strictly preserved. Fig. 3 is a reproduction on the

FIG. 3.



original scale of a photograph of a water jet taken upon 9th November. The distance recorded as 2λ is between the points marked A and B, and was of course measured upon the original negative. On each occasion when various liquids were under investigation, the photography of the water jet was repeated, and the results agreed well.

After these explanations it will suffice to summarise the actual measurements upon oleate of soda in tabular form. The standard solution contained 1 part of oleate in 40 parts of water, and was diluted as occasion required.* All lengths are given in millimetres.

	Water.	Oleate 1/40.	Oleate 1/80.	Oleate 1/400.	Oleate 1/4000.
2λ	40·0	45·5	44·0	39·0	39·0
h	31·5	11·0	11·0	11·0	23·0

In the second row h is the rise of the liquid in a capillary tube, carefully cleaned before each trial with strong sulphuric acid and copious washing. In the last case, relating to oleate solution $\frac{1}{4000}$, the motion was sluggish and the capillary height but ill-defined. It will be seen that even when the capillary height is not much more than one-third of that of water, the wave-lengths differ but little, indicating that, at any rate, the greater part of the lowering of

* Although I can find no note of the fact, I think I am right in saying that large bubbles could be blown with the weakest of the solutions experimented upon.

tension due to oleate requires time for its development. According to the law given above, the ratio of tensions of the newly-formed surfaces for water and oleate ($\frac{1}{80}$) would be merely as 6 : 5.*

Whether the slight differences still apparent in the case of the stronger solutions are due to the formation of a sensible coating in less than $\frac{1}{100}$ second, cannot be absolutely decided; but the probability appears to lie in the negative. No distinct differences could be detected between the first and second wave-lengths; but this observation is, perhaps, not accurate enough to settle the question. It is possible that a coating may be formed on the surface of the glass and metal, and that this is afterwards carried forward.

As a check upon the method, I thought it desirable to apply it to the comparison of pure water and dilute alcohol, choosing for the latter a mixture of 10 parts by volume of strong (not methylated) alcohol with 90 parts water. The results were as follows:—

$$2\lambda \text{ (water)} = 38{\cdot}5, \qquad 2\lambda \text{ (alcohol)} = 46{\cdot}5,$$
$$h \text{ (water)} = 30{\cdot}0, \qquad h \text{ (alcohol)} = 22{\cdot}0\,;$$

but it may be observed that they are not quite comparable with the preceding for various reasons, such as displacements of apparatus and changes of temperature. It is scarcely worth while to attempt an elaborate reduction of these numbers, taking into account the differences of specific gravity in the two cases; for, as was shown in the former paper, the observed values of λ are complicated by the departure of the vibrations from isochronism, when, as in the present experiments, the deviation from the circular section is moderately great. We have—

$$(46{\cdot}5/38{\cdot}5)^2 = 1{\cdot}46, \qquad 30/22 = 1{\cdot}36\,;$$

and these numbers prove, at any rate, that the method of wave-lengths is fully competent to show a change in tension, provided that the change really occurs at the first moment of the formation of the free surface.

In view of the great extensibility of saponine films it seemed important to make experiments upon this material also. The liquid employed was an infusion of horse chestnuts of specific gravity 1·02, and, doubtless, contained other ingredients as well as saponine. It was capable of giving large bubbles, even when considerably diluted (6 times) with water. Photographs taken on November 23rd gave the following results:—

* Curiously enough, I find it already recorded in my note-book of 1879, that λ is not influenced by the addition to water of soap sufficient to render impossible the rebound of colliding jets.

$$2\lambda \text{ (water)} = 39{\cdot}2, \qquad 2\lambda \text{ (saponine)} = 39{\cdot}5,$$
$$h \text{ (water)} = 30{\cdot}5, \qquad h \text{ (saponine)} = 20{\cdot}7.$$

Thus, although the capillary heights differ considerably, the tensions at the first moment are almost equal. In this case then, as in that of soap, there is strong evidence that the lowered tension is the result of the formation of a pellicle.

Though not immediately connected with the principal subject of this communication, it may be well here to record that I find saponine to have no effect inimical to the rebound after mutual collision of jets containing it. The same may be said of gelatine, whose solutions froth strongly. On the other hand, a very little soap or oleate usually renders such rebound impossible, but this effect appears to depend upon *undissolved* greasy matter. At least the drops from a nearly vertical fountain of *clear* solution of soap were found not to scatter.* The rebound of *jets* is, however, a far more delicate test than that of *drops*. A fountain of strong saponine differs in appearance from one of water; but this effect is due rather to the superficial viscosity, which retards, or altogether prevents, the resolution into drops.

The failure of rebound when jets or drops containing milk or undissolved soap come into collision has not been fully explained; but it is probably connected with the disturbance which must arise when a particle of grease from the interior reaches the surface of one of the liquid masses.

P.S.—I have lately found that the high tension of recently formed surfaces of soapy water was deduced by A. Dupré,† as long ago as 1869, from some experiments upon the vertical rise of fine jets. Although this method is less direct than that of the present paper, M. Dupré must be considered, I think, to have made out his case. It is remarkable that so interesting an observation should not have attracted more attention.

III. "On the Development of the Ciliary or Motor Oculi Ganglion." By J. C. EWART, M.D. Communicated by Professor M. FOSTER, Sec. R.S. Received February 13, 1890.

The most conflicting views have for some time been held as to the origin, relations, and homology of the ciliary (motor oculi, ophthalmic,

* 'Roy. Soc. Proc.,' June 15, 1882.

† 'Théorie Mécanique de la Chaleur,' Paris, 1869.

or lenticular) ganglion. By Remak,* Schwalbe,† Marshall,‡ and others, the ganglion of the ophthalmicus profundus has been described as the ciliary ganglion, and this ganglion has frequently been regarded as the ganglion of the motor oculi nerve, and hence as homologous with the Gasserian and other cranial ganglia. The ciliary ganglion having been shown by van Wijhe§ to be quite distinct from the ganglion of the ophthalmicus profundus, the old view of Arnold has been recently revived, and already van Wijhe, Hoffmann,|| Onodi,¶ Dohrn,** and Beard†† have indicated that they regard the ciliary as a sympathetic ganglion. Hoffmann bases his belief on certain observations on the development of the ciliary ganglion in reptiles, while Onodi has adopted this view chiefly because in the higher vertebrates the ciliary ganglion receives a communicating branch from the sympathetic. But Beard, while considering the ciliary a sympathetic ganglion, states that in sharks he has seen nothing in support of "the mode of origin for the ciliary ganglion described by Hoffmann," in reptiles.

Having examined the cranial nerves of a number of Elasmobranchs at various stages of development and growth, I am now able to give an account of the ciliary ganglion and indicate its nature and its relations to the motor oculi and other cranial nerves. In this note, however, I shall only state the more important results obtained, as the subject will be fully dealt with in a further contribution "On the Cranial Nerves of Elasmobranchs." In studying the ciliary ganglion in Elasmobranchs I have been specially struck with its tendency to vary, not only in the same genus or species, but in the same individual. Of the numerous specimens examined, I have only once found the ganglion entirely absent (in an adult *Raia radiata*), while I have occasionally (in *Acanthias*) found two well developed ganglia on each side. Usually in sharks I found the ganglion lying in connexion with the inferior branch of the motor oculi, while in skates it was generally in contact with the ophthalmicus profundus, or lying midway between the motor oculi and the ganglion of the profundus. In form the ganglion varies extremely; rounded or conical in some cases, in others it was represented by two or three

* 'Untersuchungen zur Entwickelungs-Geschichte der Wirbeltiere,' 1885.

† 'Jenaische Zeitschrift für Naturwissenschaft,' 1879, p. 173.

‡ 'Quarterly Journal Microscopical Science,' Jan., 1881.

§ "Ueber die Mesodermsegmente und die Entwickelung der Nerven des Selachierkopfes" ('Natuurk. Verh. der Koninkl. Akademie, Amsterdam,' vol. 22).

|| "Weitere Untersuchungen zur Entwickelungsgesch. der Reptilien" ('Morph. Jahrbuch,' vol. 11, part 2).

¶ 'Archiv für Anatomie und Physiologie (Physiol. Abth.),' 1887.

** "Studien zur Urgeschichte des Wirbelthierkörpers," No. 10 ('Mitteil. a. d. Zool. Stat. zu Neapel,' vol. 6, part 3).

†† 'Anatomischer Anzeiger,' Jahrgang 2 (1887), Nos. 18 and 19.

groups of cells lying parallel to or in contact with the motor oculi.

In some cases ganglionic cells had wandered from the ganglion a considerable distance along the ciliary nerves towards the eyeball.

Although in sharks the ciliary ganglion often lay in close contact with the motor oculi nerve, no ganglionic cells were ever found either in the trunk of that nerve or on any of its branches. In skates the ganglion was usually more intimately related with the ophthalmicus profundus than the oculo-motor. In all cases the ciliary ganglion had at least two roots, one from the motor oculi, and one or two from the ophthalmicus profundus. In skates the profundus root always proceeded directly from the profundus ganglion, and the profundus ganglion was frequently found to be connected by a communicating branch with the Gasserian ganglion.

Both in sharks and skates, in addition to the ciliary nerves from the ciliary ganglion there were ciliary nerves proceeding from the ganglion and from the trunk of the profundus, and in some cases large ganglionic cells had wandered from the profundus ganglion along the ciliary nerves; occasionally a few large cells had migrated some distance along the main trunk of the profundus. In all cases the majority of the cells of the ciliary ganglion were only about half the size of the cells of the profundus ganglion.

In skate embryos (*R. batis*) under two inches in length no indication of the ciliary ganglion was discovered, and in shark embryos about ten inches in length the ganglion was frequently represented by small groups of cells in the vicinity of the inferior branch of the oculomotor nerve. In sharks the first steps in the development of the ganglion were not observed, but in skates it was possible to make out all the stages. The first indication of the ganglion was in the form of a slender outgrowth from the inferior border of the large ophthalmicus profundus ganglion, which met and blended with fibres from the descending branch of the motor oculi. The *outgrowth from the profundus ganglion* was crowded with cells; the *fibres from the motor oculi*, like its root and trunk, were absolutely destitute of cells. At a somewhat later stage the cells had accumulated at the junction of the outgrowth from the profundus ganglion with the fibres from the motor oculi. It looked as if the blending of the two sets of fibres had formed a network which resisted the further migration of the ganglionic cells. In typical cases, at a still later stage, all the ganglionic cells had left the outgrowth from the profundus ganglion to form a rounded mass from which the ciliary nerves took their origin. In some instances some of the fibres which connected the profundus ganglion with the Gasserian seemed to reach and end in the ciliary ganglion. It thus appears that the ciliary ganglion stands in the same relation to one of the cranial nerves (the ophthalmicus pro-

fundus) as the sympathetic ganglia of the trunk stand to the spinal nerves, and that the ciliary ganglion may henceforth be considered a sympathetic ganglion. Further investigations may show that the ganglia in connexion with the branches of the trigeminus (fifth) nerve may also be considered as belonging to the sympathetic system. In conclusion, I may say that I have found the vestiges of the ophthalmicus profundus ganglion in a five months human embryo lying under cover of the inner portion of the Gasserian ganglion, and satisfied myself that the ophthalmicus profundus of the Elasmobranch is represented in man, as suggested by several writers, by the so-called nasal branch of the ophthalmic division of the fifth. To, as far as possible, clear up the confusion that has arisen from mistaking the ophthalmicus profundus nerve for a branch of the oculo-motor or of the trigeminus nerve, and the ganglion of the ophthalmicus profundus for the ciliary ganglion, it might be well in future to speak of the profundus as the *oculo-nasal* nerve and its ganglion as the *oculo-nasal* ganglion.

IV. "The Cranial Nerves of the Torpedo. (Preliminary Note.)" By J. C. Ewart, M.D. Communicated by Professor M. Foster, Sec. R.S. Received February 13, 1890.

The cranial nerves of the torpedo agree in their general arrangement with those of the skate.* The ophthalmicus profundus occupies the usual position, but its ganglion lies in close contact with the Gasserian, and not on a level with the ciliary, ganglion. The trigeminus has the usual distribution, for, notwithstanding the statements in the most recent text-books,† the trigeminus sends no branch to the electric organ. The facial complex includes the superficial ophthalmic, the buccal, and the hyomandibular nerves, all of which have the same distribution as the corresponding nerves in the skate; but the hyomandibular includes or is accompanied by a large bundle of nerve fibres which supply the anterior and inner portion of the electric organ. This large nerve cord (the first electric nerve) has hitherto almost invariably‡ been described as a branch of the trigeminus. When traced backwards, it is found to spring from the anterior portion of the electric lobe.

* Ewart, "On the Cranial Nerves of Elasmobranch Fishes," 'Roy. Soc. Proc.,' vol. 45, 1889.

† *E.g.*, McKendrick, 'Text-Book of Physiology,' 1888, and Wiedersheim, 'Grundriss der vergleichenden Anatomie,' 1888.

‡ Fritsch is the only author I am acquainted with who does not describe the first electric nerve as a branch of the trigeminus ('Untersuchungen über den feineren Bau des Fischgehirns,' Berlin, 1878); he, however, speaks of it as being contiguous to, and as disappearing along with, the nervus trigeminus.

The glossopharyngeus, a slender nerve in the skate, is represented in the torpedo by a thick cord which escapes by a large foramen in the outer wall of the auditory capsule. This large nerve consists of two portions, one of which is small and completely covered by the large superficial division. The small deep division, which in its course and distribution closely resembles the glossopharyngeal in the skate, presents on leaving the auditory capsule a distinct ganglionic swelling, beyond which it breaks up into the branchial and other branches. The large superficial division emanates from the electric lobe behind the origin of the first electric nerve, and at once runs outwards to reach and supply the majority of the columns of the anterior half of the electric organ.

The vagus complex consists of the nervus lateralis, the nervus intestinalis, and of five branchial nerves, of which the two anterior are accompanied by the third and fourth electric nerves. The nervus lateralis, lying superficial to all the other nerves, arises on a level with the root of the glossopharyngeus, and then curves backwards dorsal to the posterior electric nerve to reach the canal of the lateral line. Shortly after leaving the cranium it presents a distinct ganglionic swelling, which is crowded with large cells. The four branchial nerves for the four functional branchiæ, the slender filament which represents a sixth branchial nerve, and the intestinal nerve lie at first in contact with each other under cover of the third and fourth electric nerves. When the branchial and intestinal nerves are carefully examined, they are found to present four, sometimes five, ganglionic enlargements, and in addition ganglionic cells can sometimes be detected at the proximal end of the slender sixth branchial nerve. The third and fourth electric nerves lie over and are especially related to the second and third branchial nerves. These large electric nerves spring from the posterior half of the electric lobe, and find their way outwards partly behind and partly under the auditory capsule, to terminate in the posterior half of the electric organ.

It thus appears that all the electric nerves spring from the electric lobe, that the first accompanies the hyomandibular division of the facial complex, the second the glossopharyngeus, and the third and fourth the first two branchial nerves of the vagus complex. It remains to be seen whether the electric nerves have been derived from motor branches of the nerves with which they are respectively associated by an enormous increase in the number of their fibres, as the muscular fibres were gradually transformed into electric plates.

Presents, March 6, 1890.

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March 13, 1890.

Sir HENRY E. ROSCOE, Knt., Vice-President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On the Organisation of the Fossil Plants of the Coal-measures. Part XVII." By WILLIAM CRAWFORD WILLIAMSON, LL.D., F.R.S., Professor of Botany in the Owens College, Manchester. Received February 8, 1890.

(Abstract.)

In 1873 the author described in the 'Phil. Trans.' an interesting stem of a plant from the Lower Carboniferous beds of Lancashire, under the name of *Lyginodendron Oldhamium*. He also called attention to some petioles of ferns, more fully described in 1874 under the name of *Rachiopteris aspera.* The former of these plants possessed a highly organised, exogenously developed, xylem zone, whilst *Rachiopteris* was only supplied with what looked like closed bundles. Since the dates referred to, a large amount of additional information has been obtained respecting both these plants. Structures, either not seen, or at least ill-preserved, have now been discovered, throwing fresh light on their affinities; but most important of all is the proof that the *Rachiopteris aspera* is now completely identified as the foliar rachis or petiole of the *Lyginodendron;* hence there is no longer room for doubting that, notwithstanding its indisputable possession of an exogenous vascular zone, the bundles of which exhibit both xylem and phloëm elements, along with medullary and phloëm rays, it has been a true Fern. Though such exogenous developments have now been long known to exist amongst the Calamitean and Lycopodiaceous Ferns, as well as in

other plants of the Carboniferous strata, we have had no evidence until now that the same mode of growth ever occurred amongst the Ferns. Now, however, this Cryptogamic family is shown to be no longer an exceptional one in this respect. All the three great divisions of the Vascular Cryptogams, the Equisetaceæ, the Lycopodiaceæ, and the Homosporous Filices of the primæval world, exhibited the mode of growth which is confined, at the present day, to the Angiospermous plants. A further interesting feature of the life of this *Lyginodendron* is seen in the history of the development of its conspicuous medulla. In several of his previous memoirs, notably in his Part VI, the author has demonstrated a peculiarity in the origin of the medulla of the Sigillarian and Lepidodendrian plants. Instead of being a conspicuous structure in the youngest state of the stems and branches of these plants, as it is in the recent Ferns, and as in most of the living Angiosperms, few or no traces of it are observable in these fossil Lycopodiaceæ. In them it develops itself in the interior of an apparently solid bundle of tracheæ (within which doubtless some obscure cellular germs must be hidden), but ultimately it becomes a large and conspicuous organ. The author has now ascertained that a similar medulla is developed, in precisely the same way, within a large vascular bundle occupying the centre of the very young twigs of the *Lyginodendron*. But in this latter plant other phenomena associated with this development make its history even yet more clear and indisputable than in the case of the Lycopods. The entire history of these anomalous developments adds a new chapter to our records of the physiology of the vegetable kingdom.

Further light is also thrown upon the structure of the *Heterangium Grievii*, originally described in the author's memoir, Part IV. This plant presents many features in its structure suggesting that it too will ultimately prove to be a Fern. The specimens described in the above memoir, published in 1873, all possessed a more or less developed exogenous xylem zone. But the author has now obtained other examples in which no such zone exists. It is clear therefore that in this, as well as in so many others of the fossil Cryptogams of the Coal-measures, this exogenous development is a secondary phenomenon—a product of a more advanced stage of growth. In their younger states all these plants seem to approach nearer to their recent representatives than they do in their later stages of growth.

The author has now discovered the stem of a genus of plants (*Bowmanites*), hitherto known only by some fruits, the detailed organisation of which was originally described by him in the 'Transactions of the Literary and Philosophical Society of Manchester,' in 1871. The structure of this new stem corresponds closely with what is seen in *Sphenophyllum* and in some forms of *Asterophyllites* (Memoir V, 'Phil. Trans.,' 1874, p. 41, *et seq.*). This discovery makes

an addition to our knowledge of the great Calamarian family, to which the plant obviously belongs.

Further demonstrations are also given by the author, illustrating some features in the history of the true Calamites. Attention is called to the fact that, whilst the large, longitudinally-grooved and furrowed inorganic casts of the central medullary cavities of these plants are extremely common, we never find similar casts of the smaller branches. The cause of this is demonstrated in the memoir. In these young twigs the centre of the branch is at first occupied by a parenchymatous medulla. The centre of this medulla becomes absorbed at a very early age, leaving the beginnings of a small fistular cavity in its place; but, if any plastic mud or sand entered this cavity when the plant was submerged, the surface of such a cast would exhibit no longitudinal groovings, because there would be nothing in the remaining medullary cells surrounding the cast to produce such an effect. It was only when the further growth of the branch was accompanied by a more complete absorption of the remaining medullary cells, causing the cavity thus produced to be bounded by the inner wedge-shaped angles of the longitudinal vascular bundles constituting the xylem zone, that such an effect could be produced. After that change any inorganic substance finding its way into the interior of this cavity, had its surface so moulded by the wedges as to produce the superficial longitudinal ridges and furrows so characteristic of these inorganic casts.

II. "The Nitrifying Process and its Specific Ferment." By PERCY F. FRANKLAND, Ph.D., B.Sc. (Lond.), A.R.S.M., &c., Professor of Chemistry in University College, Dundee, and GRACE C. FRANKLAND. Communicated by Professor THORPE, F.R.S. Received February 28, 1890.

(Abstract.)

The process of nitrification has been practically studied for centuries, but it was first in the year 1878 that it was shown by Schloesing and Müntz to be dependent upon the presence of certain minute forms of life, or micro-organisms, or in other words to be a fermentation change.

The authors have been engaged during the last three years in endeavouring to isolate the nitrifying organism, and the present memoir gives in detail an account of the numerous experiments which were made in this direction.

Nitrification, having been in the first instance induced in a particular ammoniacal solution by means of a small quantity of garden

soil, was carried on through twenty-four generations, a minute quantity on the point of a sterilised needle being introduced from one nitrifying solution to the other. From several of these generations, gelatine-plates were poured and the resulting colonies inoculated into identical ammoniacal solutions, to see if nitrification would ensue; but, although these experiments were repeated many times, on no occasion were they successful.

It appeared, therefore, that the nitrifying organism either refused to grow in gelatine, or that the authors had failed to find it, or that, growing in gelatine, it refused to nitrify after being passed through this medium.

Experiments were, therefore, commenced to endeavour to isolate the organism by the dilution method. For this purpose a number of series of dilutions were made by the addition to sterilised distilled water of a very small quantity of an ammoniacal solution which had nitrified. It was hoped that the attenuation would be so perfect that ultimately the nitrifying organism alone would be introduced.

After a very large number of experiments had been made in this direction the authors at length succeeded in obtaining an attenuation consisting of about $\frac{1}{1000000}$ of the original nitrifying solution employed, which not only nitrified, but on inoculation into gelatine-peptone refused to grow, and was seen under the microscope to consist of numerous characteristic bacilli hardly longer than broad, which may be described as bacillo-cocci.

These results are the more striking, for in the case of the two other bottles similarly diluted, one had not nitrified, but on inoculation into gelatine-peptone produced a growth already on the second day, whilst the remaining bottle not only produced a growth, but had also nitrified, thus clearly showing that the number of organisms had been reduced to two, *i.e.*, one which nitrified and did not grow in gelatine, and another which had nothing to do with nitrification, but which grew in gelatine. In the case where nitrification took place and a growth also appeared in the gelatine-tube, it was obvious that both the nitrifying and non-nitrifying organisms, were present. These inoculation tests, together with the microscopical appearances, were confirmed by repeated experiments with invariably the same results.

It is, however, very remarkable that, although this bacillo-coccus obstinately refuses to grow in gelatine when inoculated from these dilute media, yet in broth it produces a very characteristic growth, which, although slow in commencing, often requiring three weeks before it makes its appearance, is very luxuriant.

The authors have, moreover, been successful in inducing nitrification in ammoniacal solutions inoculated from such broth cultivations, the extent of which has been quantitatively determined.

Although microscopically its form differs slightly when grown in broth and the ammoniacal solution respectively, yet its identity was established beyond question by its returning to its characteristic bacillo-coccus form when grown again in the ammoniacal solution.

The authors have also been able to induce its tardy growth in gelatine-peptone by passing it first through broth cultivations.

The paper is accompanied by carefully executed drawings of the nitrifying organism when grown in the various media employed.

Presents, March 13, 1890.

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March 20, 1890.

Sir G. GABRIEL STOKES, Bart., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The Bakerian Lecture was delivered as follows:—

BAKERIAN LECTURE.—"The Discharge of Electricity through Gases. (Preliminary Communication.)" By ARTHUR SCHUSTER, F.R.S., Professor of Physics in Owens College, Manchester. Received March 20, 1890.

[Publication deferred.]

Presents, March 20, 1890.

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March 27, 1890.

Sir G. GABRIEL STOKES, Bart., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On Black Soap Films." By A. W. REINOLD, M.A., F.R.S., and A. W. RÜCKER, M.A., F.R.S. Received March 1, 1890.

[Publication deferred.]

II. "The Variability of the Temperature of the British Isles, 1869—1883, inclusive." By ROBERT H. SCOTT, F.R.S. Received March 3, 1890.

[PLATE 9.]

The mean diurnal variability of temperature has been the subject of several papers which have appeared in the 'Zeitschrift der Oest. Gesells. für Meteorologie,' and elsewhere. Of these the most important is that by Dr. Julius Hann, entitled "Untersuchungen über die Veränderlichkeit der Tagestemperatur."* This paper contained, for ninety stations, distributed over the earth's surface, the mean diurnal variability of temperature—that is, the mean difference of the temperature of each day from that of the next—and also the frequency of a variation of 2° C., 4° C., 6° C., &c., in each month. Dr. Hann also investigated for a few stations the probability of a change of 2° C. and of 4° C.

In the case of some of the stations taken by Dr. Hann the figures compared were not daily means, but actual readings at corresponding hours on successive days. In such cases the results for variability

* 'Sitzungsberichte der K. Akad. der Wiss. in Wien,' vol. 71, 1875.

naturally comes out higher than when means for the whole day are taken.

The only British stations among the ninety were Makerstoun for five years, 1842–46, and Oxford for ten years, 1860–70 (the year 1869 being omitted). The Makerstoun means were obtained from different combinations of hours in different years, and the Oxford figures from twelve bi-hourly readings of the thermograph curves.

Inasmuch as daily mean temperatures derived from twenty-four hourly measurements of the thermograms exist at the Meteorological Office for the seven observatories during the period of their continuance, the fifteen years 1869–83 inclusive, it seemed desirable to discuss this amount of material so as to exhibit the results for these islands as an instance of a typically insular climate.

The method followed has been to extract the differences between the successive daily means, irrespective of sign, and then to take the average of the figures so obtained for each month.

The mean of these fifteen monthly values gives the mean monthly variability from the station, and this is shown in Table I.

I have appended to the tables the values given by Hann for Oxford and Makerstoun, as well as for three Continental stations, as specimens of excessive variability, and finally those for Georgetown, Demerara, as exhibiting the great constancy of temperature in that tropical locality. The last-named figures are the result of six years' observations, probably by P. Sandeman, though that is not expressly stated.

It will be seen at once that the figures for our seven observatories are much lower than those for either Oxford or Makerstoun. This may possibly be due to the fact that the periods for those two records are both of them less than fifteen years, and they are not equal to each other or synchronous.

The contrast between the British stations and the three stations of Vienna, St. Petersburg, and Barnaul is very remarkable, as is also, in the other direction, that with Georgetown, where the average on the whole year is only 1°·1 F.

Dealing with our own returns, it will be seen that the mean annual difference is greatest (2°·7) at Kew: then follow Armagh, Glasgow, and Stonyhurst with 2°·5, Aberdeen with 2°·4, and the list is closed by Falmouth and Valencia with 1°·9.

The annual range of these differences is very similar at all the seven stations, reaching a maximum in December and a minimum in August. The chief exceptions to this assertion are that at Kew the maximum of 3°·3 occurs in January and November, not in December, and that at the two south-western observatories, Falmouth and Valencia, the minimum is in July.

The highest absolute figure in any month is 5°·4, for Glasgow

Table I.—Mean Variability of Temperature.

Station.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Year.
Valencia	2·6	2·1	2·1	1·8	1·5	1·4	1·3	1·4	1·6	2·1	2·3	2·7	1·9
Armagh	3·1	2·7	2·7	2·2	2·0	1·9	2·0	1·8	2·2	2·7	3·0	3·4	2·5
Glasgow	3·0	2·7	2·6	2·1	1·9	2·0	2·0	1·7	2·1	2·8	3·1	3·4	2·5
Aberdeen	2·9	2·6	2·5	2·1	2·3	2·1	2·0	1·8	2·1	2·7	2·8	3·2	2·4
Falmouth	2·5	2·1	2·1	1·6	1·5	1·4	1·3	1·4	1·5	2·0	2·5	2·5	1·9
Stonyhurst	3·0	2·7	2·6	2·3	2·2	2·2	2·2	2·0	2·1	2·7	2·9	3·2	2·5
Kew	3·3	3·0	2·9	2·4	2·5	2·3	2·3	2·2	2·4	3·1	3·3	3·2	2·7
Oxford	3·4	3·1	2·9	3·1	3·1	2·7	2·3	2·5	2·5	3·6	3·6	3·9	3·1
Makerstoun	4·1	4·0	3·2	2·7	3·2	3·2	3·1	2·5	3·1	3·1	4·7	4·7	3·4
Vienna	3·8	3·6	3·2	3·4	3·2	3·4	3·4	3·2	3·1	2·7	3·2	3·6	3·4
St. Petersburg	5·9	5·9	5·0	3·2	4·0	3·1	2·7	2·2	3·1	3·2	4·1	5·4	4·0
Barnaul	8·8	8·5	7·2	4·7	5·6	4·3	3·4	3·2	4·5	5·6	9·0	10·1	6·3
Georgetown (Demerara)	0·9	0·9	0·7	1·1	1·4	1·3	1·4	1·3	0·7	0·9	1·3	1·3	1·1

in November, 1880, and the lowest, 0°·7, for Valencia in July, 1879.

In the detailed table at the end of this section, Table III, the mean values for each month will be found.

It has been suggested that it would be important to investigate as to whether temperature changes in these islands show more sudden positive than negative alterations. In Hindostan (Calcutta and Lahore) Mr. Blanford, in his 'Climate and Weather of India,' p. 12, states that there "rapid falls of temperature are between two and three times as frequent as rises, and, on the whole, greater in amount."

A preliminary inquiry as to all the changes during a month led to no decisive result, the number of + signs and of — signs being nearly equal. It therefore seemed best to take only the changes exceeding 5°, and, further, to mark specially those above 10°, 15°, and 20° respectively.

The following table, Table II, gives the total number of changes exceeding 5° during the entire series of years, with the mean amount of the change arranged in two sets of two columns each, marked R. and F. for rise and fall. Underneath these figures is given the number of changes exceeding 10°, &c., but without mean values.

Table II.—Variations exceeding 5°, arranged according to sign.

	Valencia.				Armagh.			
	R.		F.		R.		F.	
	No.	Mean value.	No.	Mean value.	No.	Mean value.	No.	Mean value.
	167	6·7	167	6·2	345	6·8	338	6·7
Exceeding 10°	3	..	1	..	25	..	16	
15	1							
20								

	Glasgow.				Aberdeen.			
	R.		F.		R.		F.	
	No.	Mean value.	No.	Mean value.	No.	Mean value.	No.	Mean value.
	323	7·0	346	6·7	334	7·0	325	7·0
Exceeding 10°	29	..	20	..	22	..	25	
15	..	..	..	..	..	..	2	
20	..	..	..	..	1			

Falmouth. Stonyhurst.

	Falmouth R.		Falmouth F.		Stonyhurst R.		Stonyhurst F.	
	No.	Mean value.	No.	Mean value.	No.	Mean value.	No.	Mean value.
	157	6·3	143	6·2	332	7·0	349	6·7
Exceeding 10°	2	..	3	..	24	..	19	
15	..	..	..	..	..	..	2	
20								

Kew.

	R.		F.	
	No.	Mean value.	No.	Mean value.
	430	7·1	420	6·8
Exceeding 10°	34	..	29	
15	2			
20				

It will be seen that at every observatory except Glasgow the total number of rises exceeding 5° is greater than that of falls of the same amount, and also that the mean value of the rises exceeds that of the falls, except at Aberdeen, where the two numbers are equal.

Accordingly, in these islands we have the opposite conditions to those prevailing in India, and sudden rises of temperature are more frequent and of greater amount than sudden falls.

The same fact, as regards frequency, comes out, with two slight exceptions, as regards changes exceeding 10°.

The instance exceeding 20° at Aberdeen deserves some notice. It occurred December 16, 1882, and was a rise of 23°·8. On that day at Braemar, not far from Aberdeen, the thermometer stood at 9 A.M. 44°·2 higher than at the corresponding hour on the previous day, the respective readings being −8°·3 and 35°·9. It is remarkable that this excessive change of temperature was very local, for at Dundee the difference between the successive 9 A.M. readings was only 28°·9, and at Glasgow the difference between the successive daily mean temperatures was only 12°·6, or more than 11° less than at Aberdeen.

We next come to deal with the figures for frequency in Table III.

These are obtained by arranging the changes, irrespective of sign, according to their magnitude. Six subdivisions were made 0°—0°·9, 1°·0—4°·9, 5°·0—9°·9, 10°·0—14°·9, 15°·0—19°·9, 20°·0—24°·9.

The first two of these subdivisions, taken together, cover the same range of five degrees as any one of the others, but, inasmuch as by far the greater number of changes were below 5°·0, it seemed to be worth while to ascertain how many fell short of one degree.

The total numbers were then divided by 15, the number of years, so as to obtain the mean monthly number of changes in each subdivision. The total of the figures for each month amounts to the number of days in the month.

It will be seen throughout the table how the range of changes is least at the two Atlantic stations, Falmouth and Valencia.

In every month and at every station the mean number of changes between 1°·0 and 4°·9 exceeds one-half of the number of days in the month. At Valencia, in July, the changes below 1°·0 nearly equal those between 1°·0 and 4°·9, the figures being 14°·6 and 15°·9 respectively.

Table III.—Monthly Mean Variability in each Year and Frequency of Variation.

VALENCIA.—*Variability.*

Year.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Year.
1869	2·1	2·4	1·8	2·2	1·9	1·2	1·5	1·7	2·1	1·9	3·1	2·8	1869
70	2·5	2·1	2·6	1·7	1·4	1·7	1·8	1·9	1·9	2·8	2·1	3·6	70
71	2·2	1·9	2·3	1·4	2·4	1·3	1·0	1·4	2·0	2·3	2·7	2·4	71
72	2·4	1·6	2·2	1·7	1·6	1·4	1·1	1·6	1·6	2·0	2·3	2·5	72
73	2·7	2·6	1·6	2·2	1·5	1·0	1·6	1·1	2·3	2·8	2·2	2·3	73
74	2·9	2·1	1·8	1·6	1·0	1·1	1·1	1·6	1·8	1·7	2·3	3·3	74
75	2·1	1·5	2·0	1·6	1·5	1·6	1·5	1·2	1·6	2·2	2·6	1·9	75
76	3·6	2·4	3·3	1·7	1·0	1·4	2·2	1·6	1·1	1·8	3·0	2·3	76
77	2·9	2·0	2·0	1·9	1·3	1·6	1·1	1·6	1·3	2·3	1·9	2·6	77
78	2·9	1·8	1·6	1·5	1·3	1·2	1·6	1·2	1·9	1·9	2·0	3·2	78
79	3·2	2·2	2·0	1·7	1·4	1·3	0·7	1·0	1·3	2·4	2·1	3·3	79
80	2·2	1·7	2·0	1·6	1·4	1·2	1·2	1·2	1·0	1·9	2·7	2·7	80
81	2·2	2·4	2·3	1·9	1·5	1·3	1·4	1·7	1·3	2·4	1·8	2·6	81
82	2·5	2·3	2·3	1·9	1·7	1·4	0·9	1·3	1·5	1·4	2·0	2·7	82
1883	2·5	2·4	1·9	1·7	2·2	1·6	0·9	1·3	1·1	1·9	2·2	3·0	1883
Sums ..	38·9	31·4	31·7	26·3	23·1	20·3	19·6	21·4	23·8	31·7	35·0	41·2	} 15 years.
Means ..	2·6	2·1	2·1	1·8	1·5	1·4	1·3	1·4	1·6	2·1	2·3	2·7	

Frequency.

0° — 0°·9	8·1	8·0	9·4	10·5	11·9	12·8	14·6	13·0	11·8	9·3	8·4	8·1	0° — 0°·9
1·0— 4·9	18·5	18·3	19·3	18·5	18·6	17·0	15·9	17·5	17·4	19·1	18·9	18·1	1·0— 4·9
5·0— 9·9	4·2	1·8	2·3	1·0	0·5	0·2	0·5	0·5	0·8	2·5	2·8	4·7	5·0— 9·9
10·0—14·9	0·1	..	..	..	..	..	..	0·1	..	..	..	0·1	10·0—14·9
15·0—19·9	..	..	..	..	..	..	..	..	..	..	..	0·1	15·0—19·9
20·0—24·9	..	..	..	..	..	..	..	..	..	..	..	..	20·0—24·9

Table III—*continued.*

ARMAGH.—*Variability.*

Year.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Year.
1869	2·7	2·9	2·1	3·1	1·7	2·4	2·8	2·3	2·3	3·1	4·1	3·2	1869
70	2·6	2·6	3·1	2·4	2·1	2·3	2·5	1·8	2·2	2·9	2·6	4·2	70
71	2·3	2·3	3·3	2·2	2·7	1·9	2·0	2·0	2·9	2·6	2·5	3·5	71
72	3·6	2·0	2·7	2·2	2·4	2·3	2·4	1·9	2·6	3·5	2·6	3·2	72
73	3·0	3·2	2·2	2·2	2·4	1·5	1·9	2·1	3·0	3·3	2·8	4·0	73
74	3·5	3·1	2·9	2·1	1·8	1·9	1·7	1·9	2·1	2·6	3·2	3·1	74
75	3·1	2·7	2·3	2·2	2·4	2·1	1·9	1·4	1·7	2·5	3·1	2·0	75
76	3·9	2·8	3·2	2·9	1·6	2·3	2·5	1·9	1·7	1·8	2·7	2·6	76
77	3·1	3·6	2·9	2·2	1·6	2·1	2·0	1·9	1·9	2·9	2·4	3·5	77
78	3·2	1·9	2·1	2·0	2·0	1·7	2·0	1·7	2·7	2·5	2·7	3·8	78
79	3·1	2·2	2·4	2·1	2·0	1·3	1·6	2·0	2·0	2·9	2·5	4·3	79
80	3·0	1·8	2·4	1·6	2·0	2·0	1·6	2·1	1·9	2·5	3·9	3·3	80
81	3·0	3·0	2·8	2·0	1·9	1·7	2·4	1·6	1·5	2·9	3·3	3·5	81
82	2·9	2·7	3·3	2·0	1·7	1·4	1·3	1·5	2·0	2·6	3·3	3·4	82
1883	3·0	3·4	2·4	1·7	2·3	1·6	1·0	1·5	1·9	2·4	2·8	3·7	1883
Sums ..	46·0	40·2	40·1	32·9	30·6	28·5	29·6	27·6	32·4	41·0	44·5	51·3	} 15 years.
Means ..	3·1	2·7	2·7	2·2	2·0	1·9	2·0	1·8	2·2	2·7	3·0	3·4	
						Frequency.							
0° — 0°·9	6·3	7·8	7·7	8·4	8·4	9·7	10·9	10·4	9·0	7·7	6·1	5·9	0° — 0°·9
1·0— 4·9	18·5	16·0	18·5	19·2	21·1	19·1	18·0	19·3	18·5	17·8	18·5	17·1	1·0— 4·9
5·0— 9·9	5·8	4·2	4·6	2·4	1·5	1·2	2·0	1·2	2·3	5·1	4·9	7·5	5·0— 9·9
10·0—14·9	0·5	0·2	0·2	..	..	..	0·1	0·1	0·1	0·3	0·6	0·6	10·0—14·9
15·0—19·9	..	..	..	..	..	..	..	..	..	..	..	..	15·0—19·9
20·0—24·9	..	..	..	..	..	..	..	..	..	..	..	..	20·0—24·9

Table III—*continued.*

GLASGOW.—*Variability.*

Year.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Year.
1869	2·9	3·7	2·5	3·1	1·9	2·5	2·9	1·9	2·9	3·2	4·4	3·6	1869
70	2·2	3·1	2·9	2·2	1·8	2·4	2·4	1·7	2·3	2·8	3·1	4·2	70
71	2·2	2·4	3·7	2·4	3·3	2.2	1·6	2·0	1·8	3·3	3·2	3·7	71
72	2·6	2·3	2·4	1·9	1·9	2·5	2·8	1·9	2·0	2·3	3·0	3·0	72
73	2·8	3·1	1·8	1·9	1·9	1·8	2·5	2·1	2·2	3·1	2·8	3·9	73
74	3·4	2·8	2·5	2·7	1·7	1·9	1·7	1·7	2·1	2·9	3·1	3·5	74
75	4·1	2·2	2·3	2·0	1·5	1·5	2·1	1·4	2·2	2·0	2·6	3·0	75
76	3·7	2·7	2·8	2·9	1·7	2·7	2·6	1·8	2·0	1·8	3·2	2·3	76
77	3·6	3·1	2·3	2·0	1·4	1·6	1·7	1·6	2·3	3·6	2·1	3·1	77
78	2·9	2·5	2·3	1·7	2·0	1·9	1·5	1·8	2·5	2·6	2·4	3·7	78
79	3·3	2·7	2·6	1·8	2·0	1·8	1·8	1·9	1·8	3·4	2·9	3·8	79
80	2·5	2·1	2·3	1·7	1·9	1·5	1·8	1·6	2·6	3·1	5·4	2·7	80
81	3·5	2·5	3·0	2·3	2·1	1·8	1·8	1·4	1·4	2·3	3·3	3·6	81
82	2·9	2·6	2·9	1·8	1·7	1·6	1·3	1·8	2·1	2·7	2·3	3·3	82
1883	2·8	3·0	2·4	1·6	1·9	1·8	1·5	1·4	1·9	2·3	2·4	3·2	1883
Sums ..	45·4	40·8	38·7	32·0	28·7	29·5	30·0	26·0	32·1	41·4	46·2	50·6	} 15 years.
Means ..	3·0	2·7	2·6	2·1	1·9	2·0	2·0	1·7	2·1	2·8	3·1	3·4	
						Frequency.							
0° — 0°·9	6·1	6·5	7·3	10·1	10·3	9·9	9·9	10·7	8·4	7·9	6·9	6·1	0° — 0°·9
1·0— 4·9	19·1	17·0	19·3	17·5	18·9	18·4	19·2	19·3	19·5	18·0	17·1	17·1	1·0— 4·9
5·0— 9·9	5·1	4·6	4·2	2·4	1·8	1·6	1·8	1·0	2·1	4·5	5·2	6·9	5·0— 9·9
10·0—14·9	0·7	..	0·2	0·1	..	0·1	0·1	0·1	..	0·5	0·7	0·9	10·0—14·9
15·0—19·9	..	..	..	..	..	..	..	..	..	..	..	..	15·0—19·9
20·0—24·9	..	..	..	..	..	..	..	..	..	..	..	..	20·0—24·9

Table III—*continued.*

ABERDEEN—*Variability.*

Year.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Year.
1869	2·4	3·5	2·1	3·1	2·1	2·9	2·6	2·4	2·2	3·6	3·3	3·6	1869
70	2·3	2·3	3·0	3·0	2·2	3·0	2·0	1·6	2·1	2·6	3·0	3·8	70
71	2·5	2·6	3·6	2·1	3·0	2·0	1·6	2·0	1·8	2·7	2·7	2·9	71
72	2·6	1·6	2·3	2·2	2·4	1·3	2·5	1·6	2·4	2·7	3·1	2·7	72
73	2·6	3·5	1·7	2·0	2·4	2·3	1·8	1·5	2·0	3·0	2·2	4·1	73
74	3·6	2·4	3·1	2·5	1·4	2·0	2·3	2·2	2·5	2·9	2·9	3·2	74
75	3·5	2·3	1 9	2·8	2·6	2·2	1·6	1·8	2·2	2·1	2·5	2·4	75
76	3·5	2·7	2·2	2·7	2·3	2·3	2·5	2·0	1·7	2·0	3·1	2·4	76
77	3·2	3·1	2·0	1·5	1·4	1·9	1·8	1·9	2·3	3·5	2·3	3·1	77
78	2·9	2·8	2·7	1·4	2·1	1·9	2·4	1·5	2·4	2·4	2·2	2·5	78
79	2·5	2·6	2·4	1·4	2·9	1·4	1·7	2·0	1·7	3·1	2·9	3·3	79
80	2·4	2·3	2·2	1·9	2·2	2·2	1·4	1·4	2·6	3·1	4·3	3·8	80
81	4·3	2·0	3·2	1·8	3·1	2·0	2·8	1·5	1·3	1·8	3·0	3·3	81
82	3·0	2·5	3·6	2·1	1·9	1·6	1·7	2·3	2·4	1·8	2·3	3·7	82
1883	2·7	2·6	2·0	1·6	2·6	1·9	1·7	2·0	2·2	2·7	2·2	3·0	1883
Sums ..	44·0	38·8	38·0	32·1	34·6	30·9	30·4	27·7	31·8	40·0	42·0	47·8	} 15 years.
Means ..	2·9	2·6	2·5	2·1	2·3	2·1	2·0	1·8	2·1	2·7	2·8	3·2	
						Frequency.							
0° — 0°·9	7·0	6·7	9·1	9·9	8·1	9·4	10·5	10·5	9·1	8·1	6·9	6·6	0° — 0°·9
1·0— 4·9	18·5	17·5	17·6	17·6	19·6	18·0	18·1	18·9	19·3	18·3	18·5	17·7	1·0— 4·9
5·0— 9·9	4·9	3·9	3·7	2·3	3·2	2·5	2·4	1·6	1·6	4·0	4·1	6·2	5·0— 9·9
10·0—14.9	0·6	0·1	0·4	0·2	..	0·1	..	..	..	0·5	0·5	0·5	10·0—14·9
15·0—19·9	..	..	..	..	0·1	..	..	..	..	0·1	..	..	15·0—19·9
20 0—24·9	..	..	..	..	..	..	..	..	..	..	..	0·1	20·0—24·9

Table III—*continued*.

FALMOUTH—*Variability*.

Year.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Year.
1869	1·9	2·0	2·0	1·9	1·9	1·2	1·5	2·1	1·4	1·9	3·9	2·9	1869
70	2·5	2·6	2·5	1·7	1·5	1·4	1·5	1·5	1·5	2·4	2·2	3·6	70
71	2·4	2·0	2·0	1·3	2·1	1·6	1·2	1·4	1·6	1·7	3·0	2·6	71
72	2·4	1·8	2·3	1·7	1·6	1·3	1·3	1·2	1·9	2·1	1·9	2·0	72
73	2·7	2·3	1·7	1·6	1·5	1·2	1·3	1·4	1·9	2·2	2·0	2·2	73
74	2·8	1·7	1·8	1·7	1·3	1·4	1·0	1·4	1·5	1·7	2·6	3·0	74
75	2·1	2·7	2·3	1·8	1·3	1·3	1·6	1·1	1·7	2·1	2·6	1·9	75
76	2·9	1·7	3·1	1·5	1·4	1·8	1·8	1·6	1·3	1·5	3·1	2·0	76
77	2·4	2·1	2·2	1·8	1·0	1·3	1·3	1·4	1·4	2·1	2·3	2·1	77
78	2·4	1·6	2·2	1·8	0·9	1·4	1·3	1·1	1·9	1·5	2·5	3·1	78
79	3·1	2·4	2·5	1·6	2·1	0·9	1·3	1·2	1·4	2·6	2·6	2·7	79
80	2·2	1·6	1·4	1·6	1·8	1·3	1·0	1·3	1·5	2·6	2·5	2·6	80
81	2·9	2·3	2·1	1·8	1·4	1·8	1·7	1·7	1·3	2·2	1·7	2·6	81
82	2·5	2·0	1·7	1·5	1·3	1·1	1·0	1·3	1·5	1·7	2·0	2·0	82
1883	2·3	2·5	2·2	1·4	1·8	1·9	1·0	1·3	1·3	1·6	1·9	2·6	1883
Sums ..	37·5	31·3	32·0	24·7	22·9	20·9	19·8	21·0	23·1	29·9	36·8	37·9	} 15 years.
Means ..	2·5	2·1	2·1	1·6	1·5	1·4	1·3	1·4	1·5	2·0	2·5	2·5	
						Frequency.							
0° — 0°·9	7·6	8·7	9·3	11·9	11·5	13·1	13·7	12·1	11·5	10·3	7·4	8·0	0° — 0°·9
1·0— 4·9	20·0	17·3	19·3	17·3	19·0	16·3	17·0	18·6	18·3	18·4	19·0	19·3	1·0— 4·9
5·0— 9·9	3·1	2·1	2·3	0·7	0·5	0·5	0·3	0·3	0·1	2·3	3·5	3·7	5·0— 9·9
10·0—14·6	0·3	..	..	0·1	..	..	..	..	..	..	0·1	..	10·0—14·9
15·0—19·9	..	..	..	..	..	..	..	..	..	..	..	..	15·0—19·9
20·0—24·9	..	..	..	..	..	..	..	..	..	..	..	..	20·0—24·9

Table III—*continued.*

STONYHURST—*Variability.*

Year.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Year.
1869	3·2	2·8	1·9	3·7	2·0	2·4	3·0	2·6	2·7	3·5	4·2	3·4	1869
70	2·4	3·1	2·5	2·7	2·3	2·6	2·8	2·0	2·3	2·7	2·5	4·4	70
71	3·3	3·3	3·6	2·5	3·2	2·5	1·8	2·7	2·3	2·7	2·7	2·8	71
72	2·6	2·6	3·2	2·0	1·8	2·6	2·8	2·0	2·4	2·2	2·5	2·5	72
73	3·1	3·0	2·0	2·3	1·8	2·7	3·0	2·8	3·1	3·8	2·7	3·5	73
74	3·1	2·9	2·9	2·6	2·0	1·6	1·9	2·1	1·9	2·5	3·2	3·9	74
75	3·6	2·5	2·5	1·7	2·5	2·0	2·3	1·6	2·0	2·4	3·0	2·3	75
76	3·6	2·9	2·9	2·7	1·8	2·5	2·7	2·4	1·7	2·2	2·7	2·5	76
77	3·0	3·0	2·3	1·8	1·7	2·3	1·7	2·1	2·0	2·8	2·5	2·8	77
78	2·7	2·4	2·7	1·9	2·4	2·4	1·9	1·3	2·4	2·0	2·3	3·8	78
79	3·2	1·8	2·9	1·8	2·3	1·7	1·9	1·8	2·0	3·1	3·0	3·2	79
80	2·8	2·6	2·1	1·9	1·8	2·3	1·6	1·7	2·4	2·8	4·1	2·8	80
81	3·7	2·7	2·8	2·2	2·1	1·7	2·3	1·6	1·6	2·5	2·9	3·2	81
82	2·5	2·4	2·7	2·3	2·4	1·7	1·5	1·7	1·8	2·8	2·5	3·7	82
1883	2·8	2·6	2·2	1·8	2·3	2·5	1·4	1·6	1·6	2·6	2·1	3·2	1883
Sums ..	45·6	40·6	39·2	33·9	32·4	33·5	32·6	30·0	32·2	40·6	42·9	48·0	} 15 years.
Means ..	3·0	2·7	2·6	2·3	2·2	2·2	2·2	2·0	2·1	2·7	2·9	3·2	
						Frequency.							
0°—0°·9	5·7	6·6	8·3	8·6	7·9	8·3	9·7	10·1	8·0	8·2	7·1	6·1	0°—0°·9
1·0—4·9	19·5	17·2	18·7	18·1	20·5	18·9	19·1	19·0	20·1	18·1	17·9	18·5	1·0—4·9
5·0—9·9	5·5	4·0	3·8	3·1	2·5	2·9	2·1	1·9	1·7	4·4	4·8	5·6	5·0—9·9
10·0—14·9	0·3	0·4	0·3	0·2	0·1	..	0·1	..	0·1	0·3	0·2	0·8	10·0—14·9
15·0—19·9	..	..	..	..	..	..	..	0·1	..	..	0·1	..	15·0—19·9
20·0—24·9	..	..	..	..	..	..	..	..	..	..	..	..	20·0—24·9

Table III—*continued.*

KEW—*Variability.*

Year.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Year.
1869	3·2	3·5	2·2	3·3	2·9	2·7	3·4	3·0	3·1	3·8	4·7	3·2	1869
70	3·2	2·8	3·7	3·0	3·4	3·3	2·9	2·0	3·1	3·7	3·1	3·7	70
71	2·7	3·4	3·4	2·1	3·0	1·9	1·8	2·4	2·4	3·0	2·6	2·3	71
72	2·7	2·9	2·8	3·3	2·7	2·8	3·3	2·1	3·6	3·0	2·6	2·5	72
73	2·3	2·7	2·6	2·7	2·3	2·4	2·6	2·3	2·4	4·0	2·8	3·8	73
74	4·4	3·8	3·1	2·4	1·9	2·5	2·5	2·3	2·3	2·9	4·1	3·6	74
75	3·3	2·6	2·7	2·3	2·4	2·2	2·0	1·7	2·3	2·7	3·4	2·1	75
76	4·5	2·8	3·7	2·4	1·6	2·4	2·4	2·8	1·9	2·4	3·0	2·5	76
77	3·9	3·1	2·7	1·9	2·1	2·5	2·2	2·7	2·0	3·4	3·7	3·7	77
78	3·5	2·5	2·5	1·5	2·2	2·2	1·7	1·4	2·4	2·4	2·9	2·7	78
79	2·9	2·7	3·2	2·3	2·6	1·2	1·6	2·1	1·8	3·4	3·3	4·3	79
80	2·9	2·8	2·7	2·0	2·9	2·1	1·2	1·9	2·3	3·9	4·3	4·5	80
81	3·1	3·3	3·0	2·7	2·6	2·1	3·3	2·1	1·9	2·8	3·1	2·8	81
82	3·3	3·3	2·7	2·0	2·3	1·9	1·6	1·9	2·0	2·7	3·2	3·9	82
1883	3·2	3·3	2·5	2·6	2·9	2·9	1·9	2·3	2·0	2·3	3·4	2·9	1883
Sums ..	49·1	45·5	43·5	36·5	37·8	35·1	34·4	33·0	35·5	46·4	50·2	48·5	} 15 years.
Means ..	3·3	3·0	2·9	2·4	2·5	2·3	2·3	2·2	2·4	3·1	3·3	3·2	
						Frequency.							
0° — 0°·9	6·3	5·3	6·5	8·1	8·3	8·8	8·2	7·9	7·3	6·5	5·9	6·8	0° — 0°·9
1·0— 4·9	17·4	17·6	19·5	18·6	18·5	18·1	19·6	20·9	19·9	18·3	16·9	17·5	1·0— 4·9
5·0— 9·9	6·5	5·1	4·6	3·1	4·1	3·1	3·1	2·1	2·8	5·7	6·4	5·7	5·0— 9·9
10·0—14·9	0·7	0·2	0·3	0·2	0·1	0·1	0·1	0·1	0·1	0·5	0·8	1·0	10·0—14·9
15·0—19·9	..	0·1	..	..	..	..	..	..	..	..	..	0·1	15·0—19·9
20·0—24·9	..	..	..	..	..	..	..	..	..	..	..	..	20·0—24·9

When dealing with the daily mean values for the observatories, it seemed worth while to append to the paper a notice of the distribution of these mean values. They have been arranged in seven columns according to their heights, 10°·0—19°·9, 20°·0—31°·9, 32°·0—39°·9, 40°·0—49°·9, 50°·0—59°·9, 60°·0—69°·9, 70°·0—79°·9. These intervals are naturally unequal, the exigencies of the Fahrenheit scale not suiting the decimal division about the freezing point.

These figures are given in detail for the different years in Table IV, in order to give a general idea of the character of each year.

Taking the winters, we see that Stonyhurst had in the severe winter of 1881 four days in January on which the mean temperature did not reach 20°, and had 19 days in all in that month in which the mean temperature did not rise to the freezing point.

In the same month the number of days with a mean below 32° was higher (21) than at Stonyhurst both at Aberdeen and Glasgow, but the cold was not so intense at these stations as at Stonyhurst, for the number of days with a mean below 20°·0 was less.

Neither at Falmouth nor at Valencia did the mean ever fall below 20°·0.

Conversely, as regards higher temperatures, Kew far outstrips the other stations, July showing in the interval of 15 years 35 days on which the mean temperature amounted to 70°·0 or upwards.

Table IV.—Number of occasions in each Month and each Year on which the Mean Daily Temperature reached definite limits.

VALENCIA.

January.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·0.
1869	..	..	..	24	7	..	..
70	..	..	5	24	2	..	..
71	..	..	5	26	..	..	..
72	..	..	1	26	4	..	..
73	..	..	2	26	3	..	..
74	..	..	1	26	4	..	..
75	..	..	..	17	14	..	..
76	..	..	8	12	11	..	..
77	..	..	1	30	..	..	..
78	..	..	2	21	8	..	..
79	..	..	13	18	..	..	..
80	..	..	7	23	1	..	..
81	..	5	13	13	..	..	..
82	..	..	..	22	9	..	..
1883	..	..	2	26	3	..	..
Sums	..	5	60	334	66	..	..

Table IV—*continued.*

Valencia—*continued.*

February.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	18	10	..	..
70	..	..	11	17	..	..	..
71	..	..	..	23	5	..	..
72	..	..	..	27	2	..	..
73	..	1	7	20	..	..	..
74	..	..	..	26	2	..	..
75	..	..	9	17	2	..	..
76	..	..	3	20	6	..	..
77	..	..	..	20	8	..	..
78	..	..	2	19	7	..	..
79	..	..	7	21	..	..	..
80	..	..	..	27	2	..	..
81	..	..	7	21	..	..	..
82	..	..	..	24	4	..	..
1883	..	..	1	25	2	..	..
Sums	..	1	47	325	50	..	..

March.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	2	28	1	..	..
70	..	..	3	22	6	..	..
71	..	..	1	21	9	..	..
72	..	..	3	17	11	..	..
73	..	..	2	28	1	..	..
74	..	..	3	17	11	..	..
75	..	..	4	26	1	..	..
76	..	..	3	26	2	..	..
77	..	..	..	29	2	..	..
78	..	..	..	20	11	..	..
79	..	..	3	27	1	..	..
80	..	..	..	25	6	..	..
81	..	..	3	23	5	..	..
82	..	..	..	21	10	..	..
1883	..	..	8	23	..	..	..
Sums	..	..	35	353	77	..	..

Table IV—*continued.*

VALENCIA—*continued.*

April.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	9	19	2	..
70	..	..	..	19	11	..	..
71	..	..	..	11	19	..	..
72	..	..	..	20	10	..	..
73	..	..	..	12	18	..	..
74	..	..	..	14	16	..	..
75	..	..	..	15	15	..	..
76	..	..	1	14	15	..	..
77	..	..	..	21	9	..	..
78	..	..	..	10	20	..	..
79	..	..	1	27	2	..	..
80	..	..	..	22	8	..	..
81	..	..	..	13	17	..	..
82	..	..	..	18	12	..	..
1883	..	..	..	26	4	..	..
Sums	..	..	2	251	195	2	..

May.

1869	..	..	..	12	19	..	..
70	..	..	..	6	25	..	..
71	..	..	..	3	24	4	..
72	..	..	..	15	16	..	..
73	..	..	..	7	24	..	..
74	..	..	..	6	25	..	..
75	..	..	..	1	30	..	..
76	..	..	..	5	26	..	..
77	..	..	..	9	22	..	..
78	..	..	..	1	30	..	..
79	..	..	..	13	18	..	..
80	..	..	..	6	24	1	..
81	..	..	..	10	21	..	..
82	..	..	..	6	25	..	..
1883	..	..	..	8	23	..	..
Sums	..	..	..	108	352	5	..

Table IV—*continued.*

VALENCIA—*continued.*

June.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	..	24	6	..
70	..	..	..	..	24	6	..
71	..	..	..	..	29	1	..
72	..	..	..	2	27	1	..
73	..	..	..	..	29	1	..
74	..	..	..	..	25	5	..
75	..	..	..	..	29	1	..
76	..	..	..	..	30	..	..
77	..	..	..	..	23	7	..
78	..	..	..	..	28	2	..
79	..	..	..	..	30	..	..
80	..	..	..	..	27	3	..
81	..	..	..	1	28	1	..
82	..	..	..	..	28	2	..
1883	..	..	..	..	26	4	..
Sums	..	..	..	3	407	40	..

July.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	..	8	23	..
70	..	..	..	..	14	17	..
71	..	..	..	..	24	7	..
72	..	..	..	..	15	16	..
73	..	..	..	..	25	6	..
74	..	..	..	..	19	12	..
75	..	..	..	..	24	7	..
76	..	..	..	..	20	11	..
77	..	..	..	..	28	3	..
78	..	..	..	..	6	25	..
79	..	..	..	..	31	..	..
80	..	..	..	..	23	8	..
81	..	..	..	..	25	6	..
82	..	..	..	..	31	..	..
1883	..	..	..	..	31	..	..
Sums	..	..	..	..	324	141	..

Table IV—*continued.*

Valencia—*continued.*

August.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	..	20	10	1
70	..	..	..	..	5	26	..
71	..	..	..	..	13	18	..
72	..	..	..	..	20	11	..
73	..	..	..	..	24	7	..
74	..	..	..	..	21	10	..
75	..	..	..	..	11	20	..
76	..	..	..	..	18	13	..
77	..	..	..	..	22	9	..
78	..	..	..	..	7	24	..
79	..	..	..	..	29	2	..
80	..	..	..	..	5	26	..
81	..	..	..	..	29	2	..
82	..	..	..	..	23	8	..
1883	..	..	..	..	28	3	..
Sums	..	..	..	..	275	189	1

September.

1869	..	..	..	..	25	5	..
70	..	..	..	..	22	8	..
71	..	..	..	2	24	4	..
72	..	..	..	1	19	10	..
73	..	..	..	1	28	1	..
74	..	..	..	1	29	..	..
75	..	..	..	..	14	16	..
76	..	..	..	..	30	..	..
77	..	..	..	..	30	..	..
78	..	..	..	..	18	12	..
79	..	..	..	..	30	..	..
80	..	..	..	..	17	13	..
81	..	..	..	..	30	..	..
82	..	..	..	1	29	..	..
1883	..	..	..	..	30	..	..
Sums	..	..	..	6	375	69	..

Table IV—*continued.*

VALENCIA—*continued.*

October.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	6	19	6	..
70	..	..	..	7	21	3	..
71	..	..	..	4	27	..	..
72	..	..	..	19	12	..	..
73	..	..	..	14	17	..	..
74	..	..	..	4	27	..	..
75	..	..	..	11	20	..	..
76	..	..	..	3	28	..	..
77	..	..	..	4	27	..	..
78	..	..	..	10	19	2	..
79	..	..	..	12	19	..	..
80	..	..	..	25	6	..	..
81	..	..	..	9	22	..	..
82	..	..	..	8	23	..	..
1883	..	..	..	6	25	..	..
Sums	..	..	..	142	312	11	..

November.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	13	17	..	..
70	..	..	..	25	5	..	..
71	..	..	3	19	8	..	..
72	..	..	3	21	6	..	..
73	..	..	..	23	7	..	..
74	..	..	..	18	12	..	..
75	..	..	6	13	11	..	..
76	..	..	..	14	16	..	..
77	..	..	..	21	9	..	..
78	..	..	5	25	..	..	..
79	..	..	4	18	8	..	..
80	..	..	3	14	13	..	..
81	..	..	..	10	20	..	..
82	..	..	..	21	9	..	..
1883	..	..	..	21	9	..	..
Sums	..	..	24	276	150	..	..

Table IV—*continued*.

Valencia—*continued*.

December.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	1	5	24	1	..	..
70	..	..	14	16	1	..	..
71	..	..	6	22	3	..	..
72	..	..	1	26	4	..	..
73	..	..	..	15	16	..	..
74	..	..	4	27	..	..	..
75	..	..	10	13	8	..	..
76	..	..	1	23	7	..	..
77	..	..	..	27	4	..	..
78	..	2	16	11	2	..	..
79	..	..	7	19	5	..	..
80	..	..	4	14	13	..	..
81	..	..	3	25	3	..	..
82	..	..	9	16	6	..	..
1883	..	..	3	23	5	..	..
Sums	..	3	83	301	78	..	..

Armagh.

January.

1869	..	..	6	25	..	..	..
70	..	1	14	16	..	..	..
71	..	1	26	4	..	..	..
72	..	1	14	15	1	..	..
73	..	1	13	16	1	..	..
74	..	..	9	22	..	..	..
75	..	..	4	27	..	..	..
76	..	2	11	16	2	..	..
77	..	..	11	20	..	..	..
78	..	..	14	17	..	..	..
79	..	12	16	3	..	..	..
80	..	5	13	12	1	..	..
81	..	19	5	7	..	..	..
82	..	..	9	20	2	..	..
1883	..	..	10	20	1	..	..
Sums	..	42	175	240	8	..	..

Table IV—*continued.*

ARMAGH—*continued.*

February.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	2	22	4	..	..
70	..	5	11	12	..	..	..
71	..	..	3	24	1	..	..
72	..	..	1	28	..	..	..
73	..	4	15	9	..	..	..
74	..	1	9	18	..	..	..
75	..	..	18	9	1	..	..
76	..	2	12	15	..	..	..
77	..	..	9	18	1	..	..
78	..	..	4	24	..	..	..
79	..	..	19	9	..	..	..
80	..	..	6	23	..	..	..
81	..	1	13	14	..	..	..
82	..	..	1	25	2	..	..
1883	..	..	9	19	..	..	..
Sums	..	13	132	269	9	..	..

March.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	17	14	..	..	..
70	..	1	13	12	5	..	..
71	..	..	8	18	5	..	..
72	..	..	8	18	5	..	..
73	..	..	15	13	3	..	..
74	..	2	1	25	3	..	..
75	..	..	11	20	..	..	..
76	..	..	20	11	..	..	..
77	..	..	14	16	1	..	..
78	..	..	14	17	..	..	..
79	..	..	15	16	..	..	..
80	..	..	3	26	2	..	..
81	..	..	14	13	4	..	..
82	..	..	2	27	2	..	..
1883	..	..	25	6	..	..	..
Sums	..	3	180	252	30	..	..

Table IV—*continued.*

ARMAGH—*continued.*

April.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	2	18	10	..	..
70	..	..	..	21	9	..	..
71	..	..	..	25	5	..	..
72	..	..	2	22	6	..	..
73	..	..	..	24	6	..	..
74	..	..	..	17	11	2	..
75	..	..	1	21	8	..	..
76	..	..	5	18	7	..	..
77	..	..	..	30	..	..	..
78	..	..	4	14	12	..	..
79	..	..	6	24	..	..	..
80	..	..	..	27	3	..	..
81	..	..	9	17	4	..	..
82	..	..	1	25	4	..	..
1883	..	..	..	29	1	..	..
Sums	..	..	30	332	86	2	..

May.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	26	5	..	..
70	..	..	..	11	19	1	..
71	..	..	..	10	21	..	..
72	..	..	..	20	11	..	..
73	..	..	..	16	15	..	..
74	..	..	..	17	14	..	..
75	..	..	..	5	25	1	..
76	..	..	..	16	15	..	..
77	..	..	..	20	11	..	..
78	..	..	..	10	21	..	..
79	..	..	1	23	7	..	..
80	..	..	..	13	18	..	..
81	..	..	..	9	21	1	..
82	..	..	..	14	17	..	..
1883	..	..	1	11	19	..	..
Sums	..	..	2	221	239	3	..

Table IV—*continued.*

ARMAGH—*continued.*

June.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	4	23	3	..
70	..	..	..	..	25	5	..
71	..	..	..	..	28	2	..
72	..	..	..	6	20	4	..
73	..	..	..	..	26	4	..
74	..	..	..	..	28	2	..
75	..	..	..	..	29	1	..
76	..	..	..	2	24	4	..
77	..	..	..	1	22	7	..
78	..	..	..	..	23	7	..
79	..	..	..	4	26	..	..
80	..	..	..	5	22	3	..
81	..	..	..	5	23	2	..
82	..	..	..	3	26	1	..
1883	..	..	..	2	28	..	..
Sums	..	..	..	32	373	45	..

July.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	..	12	19	..
70	..	..	..	..	15	16	..
71	..	..	..	..	27	4	..
72	..	..	..	..	14	17	..
73	..	..	..	..	23	8	..
74	..	..	..	..	19	12	..
75	..	..	..	..	28	3	..
76	..	..	..	..	22	9	..
77	..	..	..	1	28	2	..
78	..	..	..	..	19	12	..
79	..	..	..	..	29	2	..
80	..	..	..	..	30	1	..
81	..	..	..	..	22	9	..
82	..	..	..	..	28	3	..
1883	..	..	..	..	31	..	..
Sums	..	..	..	1	347	117	..

Table IV—*continued.*

Armagh—*continued.*

August.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	1	22	8	..
70	..	..	..	..	16	15	..
71	..	..	..	..	18	13	..
72	..	..	..	..	22	9	..
73	..	..	..	..	27	4	..
74	..	..	..	..	22	9	..
75	..	..	..	..	20	11	..
76	..	..	..	..	19	12	..
77	..	..	..	..	25	6	..
78	..	..	..	..	16	15	..
79	..	..	..	2	26	3	..
80	..	..	..	..	14	17	..
81	..	..	..	..	27	4	..
82	..	..	..	..	22	9	..
1883	..	..	..	..	29	2	..
Sums	..	..	..	3	325	137	..

September.

1869	..	..	..	2	26	2	..
70	..	..	..	..	30	..	..
71	..	..	..	11	18	1	..
72	..	..	..	10	14	6	..
73	..	..	..	22	8	..	..
74	..	..	..	4	26	..	..
75	..	..	..	..	26	4	..
76	..	..	..	4	26	..	..
77	..	..	..	8	22	..	..
78	..	..	..	5	21	4	..
79	..	..	..	5	25	..	..
80	..	..	..	2	23	5	..
81	..	..	..	2	28	..	..
82	..	..	..	9	21	..	..
1883	..	..	..	2	28	..	..
Sums	..	..	..	86	342	22	..

Table IV—*continued.*

ARMAGH—*continued.*

October.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	4	11	13	3	..
70	..	..	..	21	10	..	..
71	..	..	..	19	12	..	..
72	..	..	2	25	4	..	..
73	..	..	6	19	5	1	..
74	..	..	..	21	10	..	..
75	..	..	..	20	11	..	..
76	..	..	..	9	22	..	..
77	..	..	..	16	15	..	..
78	..	..	2	13	15	1	..
79	..	..	..	26	5	..	..
80	..	..	9	21	1	..	..
81	..	..	4	20	7	..	..
82	..	..	1	15	15	..	..
1883	..	..	..	22	9	..	..
Sums	..	..	28	278	154	5	..

November.

1869	..	..	10	14	6	..	..
70	..	..	15	15	..	..	..
71	..	..	15	15	..	..	..
72	..	..	11	17	2	..	..
73	..	..	8	20	2	..	..
74	..	..	4	23	3	..	..
75	..	..	17	8	5	..	..
76	..	..	8	19	3	..	..
77	..	..	6	23	1	..	..
78	..	2	22	6	..	..	..
79	..	..	10	17	3	..	..
80	..	2	10	15	3	..	..
81	..	..	..	19	11	..	..
82	..	..	18	11	1	..	..
1883	..	2	10	16	2	..	..
Sums	..	6	164	238	42	..	..

Table IV—*continued.*

Armagh—*continued.*

December.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	69°—69°·9.	70°—79°·9.
1869	..	5	16	10	..	..	..
70	1	12	10	8	..	..	..
71	..	2	12	17	..	..	..
72	..	1	15	14	1	..	..
73	..	..	4	25	2	..	..
74	..	7	21	3	..	..	..
75	..	7	6	18	..	..	..
76	..	..	8	23	..	..	..
77	..	..	12	19	..	..	..
78	1	16	11	3	..	..	..
79	..	12	8	11	..	..	..
80	..	4	12	13	2	..	..
81	..	4	14	13	..	..	..
82	..	10	10	11	..	..	..
1883	..	1	10	20	..	..	..
Sums	2	81	169	208	5	..	..

Glasgow.

January.

1869	..	..	9	22	..	..	..
70	..	1	21	9	..	..	..
71	..	7	21	3	..	..	..
72	..	..	13	17	1	..	..
73	..	2	12	17	..	..	..
74	..	..	10	21	..	..	..
75	..	1	11	19	..	..	..
76	..	2	11	18	..	..	..
77	..	3	19	9	..	..	..
78	..	2	15	14	..	..	..
79	..	17	13	1	..	..	..
80	..	7	12	11	1	..	..
81	3	18	7	3	..	..	..
82	..	..	7	24	..	..	..
1883	..	..	17	14	..	..	..
Sums	3	60	198	202	2	..	..

Table IV—*continued.*

Glasgow—*continued.*

February.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	7	19	2	..	..
70	..	6	15	7	..	..	..
71	..	..	9	19	..	..	..
72	..	..	7	22	..	..	..
73	..	8	14	6	..	..	..
74	..	1	11	16	..	..	..
75	..	2	20	6	..	..	..
76	..	3	18	8	..	..	..
77	..	2	8	18	..	..	..
78	..	..	9	19	..	..	..
79	..	7	18	3	..	..	..
80	..	..	6	23	..	..	..
81	..	2	24	2	..	..	..
82	..	..	4	24	..	..	..
1882	..	..	9	19	..	..	..
Sums	..	31	179	211	2	..	..

March.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
69	..	..	19	12	..	..	..
70	..	1	15	15	..	..	..
71	..	1	7	22	1	..	..
72	..	..	14	15	2	..	..
73	..	..	18	13	..	..	..
74	..	2	2	25	2	..	..
75	..	..	16	15	..	..	..
76	..	3	15	13	..	..	..
77	..	..	21	10	..	..	..
78	..	..	16	15	..	..	..
79	..	4	15	12	..	..	..
80	..	..	12	19	..	..	..
81	..	5	14	12	..	..	..
82	..	..	7	24	..	..	..
1883	..	..	26	5	..	..	..
Sums	..	16	217	227	5	..	..

Table IV—*continued.*

Glasgow—*continued.*

April.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	3	19	8	..	..
70	..	..	..	24	6	..	..
71	..	..	4	25	1	..	..
72	..	..	3	22	5	..	..
73	..	..	2	25	3	..	..
74	..	..	..	20	10	..	..
75	..	..	1	22	7	..	..
76	..	..	7	18	5	..	..
77	..	..	7	23	..	..	..
78	..	..	2	20	8	..	..
79	..	..	11	19	..	..	..
80	..	..	..	29	1	..	..
81	..	..	10	18	2	..	..
82	..	..	2	26	2	..	..
1883	..	..	1	28	1	..	..
Sums	..	..	53	338	59	..	..

May.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	1	28	2	..	..
70	..	..	..	11	20	..	..
71	..	..	..	14	12	5	..
72	..	..	..	23	8	..	..
73	..	..	2	18	11	..	..
74	..	..	..	22	9	..	..
75	..	..	..	6	25	..	..
76	..	..	..	14	17	..	..
77	..	..	2	20	9	..	..
78	..	..	..	12	19	..	..
79	..	..	..	28	3	..	..
80	..	..	..	18	13	..	..
81	..	..	..	15	13	3	..
82	..	..	..	14	17	..	..
1883	..	..	1	14	16	..	..
Sums	..	..	6	257	194	8	..

Table IV—*continued.*

GLASGOW—*continued.*

June.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	6	20	4	..
70	..	..	..	2	24	4	..
71	..	..	..	2	28	..	..
72	..	..	..	4	22	4	..
73	..	..	..	..	29	1	..
74	..	..	..	..	30	..	..
75	..	..	..	..	30	..	..
76	..	..	..	..	24	6	..
77	..	..	..	1	29	..	..
78	..	..	..	2	21	5	2
79	..	..	..	5	25	..	..
80	..	..	..	5	23	2	..
81	..	..	..	5	23	2	..
82	..	..	..	3	26	1	..
1883	..	..	..	1	28	1	..
Sums	..	..	..	36	382	30	2

July

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	..	17	14	..
70	..	..	..	..	19	10	2
71	..	..	..	..	27	4	..
72	..	..	..	..	16	15	..
73	..	..	..	..	27	3	1
74	..	..	..	..	21	10	..
75	..	..	..	..	25	6	..
76	..	..	..	..	25	6	..
77	..	..	..	..	30	1	..
78	..	..	..	..	18	13	..
79	..	..	..	..	29	2	..
80	..	..	..	..	27	4	..
81	..	..	..	..	30	1	..
82	..	..	..	..	27	4	..
1883	..	..	..	..	26	5	..
Sums	..	..	..	..	364	98	3

Table IV—*continued*

GLASGOW—*continued.*

August.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	2	23	6	..
70	..	..	..	1	16	14	..
71	..	..	..	..	21	10	..
72	..	..	..	..	29	2	..
73	..	..	..	..	30	1	..
74	..	..	..	..	29	2	..
75	..	..	..	..	26	5	..
76	..	..	..	..	22	9	..
77	..	..	..	2	25	4	..
78	..	..	..	..	24	7	..
79	..	..	..	1	27	3	..
80	..	..	..	..	18	13	..
81	..	..	..	4	26	1	..
82	..	..	..	..	26	5	..
1883	..	..	..	..	31	..	..
Sums	..	..	..	10	373	82	..

September.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	4	25	1	..
70	..	..	..	2	28	..	..
71	..	..	..	12	18	..	..
72	..	..	..	12	18	..	..
73	..	..	..	9	21	..	..
74	..	..	..	6	24	..	..
75	..	..	..	5	23	2	..
76	..	..	..	7	23	..	..
77	..	..	..	13	17	..	..
78	..	..	..	8	19	3	..
79	..	..	..	8	22	..	..
80	..	..	..	3	24	3	..
81	..	..	..	1	29	..	..
82	..	..	..	7	23	..	..
1883	..	..	..	3	27	..	..
Sums	..	..	..	100	341	9	..

Table IV—*continued.*

Glasgow—*continued.*

October.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	6	11	13	1	..
70	..	..	..	27	4	..	..
71	..	..	2	19	10	..	..
72	..	..	2	26	3	..	..
73	..	..	5	21	5	..	..
74	..	..	1	26	4	..	..
75	..	..	1	23	7	..	..
76	..	..	1	14	16	..	..
77	..	..	2	18	11	..	..
78	..	..	2	11	18	..	..
79	..	..	4	22	5	..	..
80	..	..	9	20	2	..	..
81	..	..	5	19	7	..	..
82	..	..	2	14	15	..	..
1883	..	..	..	25	6	..	..
Sums	..	..	42	296	126	1	..

November.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	4	11	11	4	..	..
70	..	1	18	11	..	..	..
71	..	1	17	12	..	..	..
72	..	..	10	17	3	..	..
73	..	2	7	20	1	..	..
74	..	..	10	16	4	..	..
75	..	2	17	8	3	..	..
76	..	5	11	12	2	..	..
77	..	..	8	21	1	..	..
78	..	..	18	12	..	..	..
79	..	2	11	16	1	..	..
80	..	6	8	15	1	..	..
81	..	..	3	21	6	..	..
82	..	..	20	10	..	..	..
1883	..	2	14	12	2	..	..
Sums	..	25	183	214	28	..	..

Table IV—*continued.*

GLASGOW—*continued.*

December.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	8	16	7	..	..	..
70	2	7	17	5	..	..	..
71	..	3	14	14	..	..	..
72	..	3	15	13	..	..	..
73	..	..	7	24	..	..	..
74	1	14	14	2	..	..	..
75	..	4	7	20	..	..	..
76	..	1	8	22	..	..	..
77	..	2	9	20	..	..	..
78	1	14	14	2	..	..	..
79	1	8	11	11	..	..	..
80	..	5	13	12	1	..	..
81	..	2	16	13	..	..	..
82	..	11	11	9	..	..	..
1883	..	11	20	..	..	..	..
Sums	5	93	192	174	1	..	..

ABERDEEN.

January.

1869	..	..	11	20	..	..	..
70	..	1	24	6	..	..	..
71	..	7	22	2	..	..	..
72	..	..	15	16	..	..	..
73	..	..	15	16	..	..	..
74	..	..	12	19	..	..	..
75	..	2	12	17	..	..	..
76	..	5	9	17	..	..	..
77	..	2	19	10	..	..	..
78	..	3	20	8	..	..	..
79	..	10	20	1	..	..	..
80	..	2	18	11	..	..	..
81	1	20	8	2	..	..	..
82	..	..	11	20	..	..	..
1883	..	..	19	12	..	..	..
Sums	1	52	235	177	..	..	..

Table IV—*continued.*

Aberdeen—*continued.*

February.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	1	9	16	2	..	..
70	..	7	14	7	..	..	..
71	..	..	10	17	1	..	..
72	..	..	9	20	..	..	..
73	..	5	19	4	..	..	..
74	..	1	13	14	..	..	..
75	..	2	20	6	..	..	..
76	..	3	22	4	..	..	..
77	..	3	13	12	..	..	..
78	..	..	10	18	..	..	..
79	..	6	19	3	..	..	..
80	..	..	7	22	..	..	..
81	..	3	24	1	..	..	..
82	..	2	9	17	..	..	..
1883	..	..	7	21	..	..	..
Sums	..	33	205	182	3	..	..

March.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	1	22	8	..	..	..
70	..	3	12	16	..	..	..
71	..	1	8	21	1	..	..
72	..	..	14	17	..	..	..
73	..	..	18	13	..	..	..
74	..	3	3	22	3	..	..
75	..	..	19	12	..	..	..
76	..	5	15	11	..	..	..
77	..	..	23	8	..	..	..
78	..	6	14	10	1	..	..
79	..	3	17	11	..	..	..
80	..	..	12	19	..	..	..
81	..	6	19	5	1	..	..
82	..	..	7	23	1	..	..
1883	..	8	19	4	..	..	..
Sums	..	36	222	200	7	..	..

Table IV—*continued.*

ABERDEEN—*continued.*

April.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	5	15	10	..	..
70	..	..	2	25	3	..	..
71	..	..	11	19	..	..	..
72	..	..	7	18	5	..	..
73	..	..	3	26	1	..	..
74	..	..	..	25	5	..	..
75	..	..	1	25	4	..	..
76	..	..	7	20	3	..	..
77	..	..	12	18	..	..	..
78	..	..	5	22	3	..	..
79	..	..	18	12	..	..	..
80	..	..	1	28	1	..	..
81	..	..	15	15	..	..	..
82	..	1	7	20	2	..	..
1883	..	..	..	30	..	..	..
Sums	..	1	94	318	37	..	..

May.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	1	29	1	..	..
70	..	..	1	13	17	..	..
71	..	..	1	16	14	..	..
72	..	..	..	23	8	..	..
73	..	..	2	26	3	..	..
74	..	..	1	25	5	..	..
75	..	..	..	15	15	1	..
76	..	..	1	22	8	..	..
77	..	..	5	24	2	..	..
78	..	..	..	20	11	..	..
79	..	..	4	25	2	..	..
80	..	..	..	20	11	..	..
81	..	..	1	15	15	..	..
82	..	..	..	24	7	..	..
1883	..	..	3	17	10	1	..
Sums	..	..	20	314	129	2	..

Table IV—*continued.*

ABERDEEN—*continued.*

June.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	10	18	2	..
70	..	..	..	3	24	3	..
71	..	..	..	15	15	..	..
72	..	..	..	2	27	1	..
73	..	..	..	..	28	2	..
74	..	..	..	4	25	1	..
75	..	..	..	2	27	1	..
76	..	..	..	2	27	1	..
77	..	..	..	4	26	..	..
78	..	..	..	12	13	5	..
79	..	..	..	15	15	..	..
80	..	..	..	6	23	1	..
81	..	..	..	13	17	..	..
82	..	..	..	8	22	..	..
1883	..	..	..	11	18	1	..
Sums	..	..	..	107	325	18	..

July.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	..	22	9	..
70	..	..	..	..	22	9	..
71	..	..	..	..	26	5	..
72	..	..	..	..	23	8	..
73	..	..	..	..	26	5	..
74	..	..	..	..	21	10	..
75	..	..	..	1	29	1	..
76	..	..	..	..	25	6	..
77	..	..	..	1	27	3	..
78	..	..	..	..	26	4	1
79	..	..	..	1	30	..	..
80	..	..	..	..	31	..	..
81	..	..	..	1	25	5	..
82	..	..	..	..	29	2	..
1883	..	..	..	..	27	4	..
Sums	..	..	..	4	389	71	1

Table IV—*continued.*

ABERDEEN—*continued.*

August.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	1	26	4	..
70	..	..	..	1	25	5	..
71	..	..	..	..	21	10	..
72	..	..	..	..	30	1	..
73	..	..	..	1	29	1	..
74	..	..	..	1	28	2	..
75	..	..	..	..	27	4	..
76	..	..	..	2	27	2	..
77	..	..	..	3	28	..	..
78	..	..	..	..	28	3	..
79	..	..	..	1	30	..	..
80	..	..	..	..	25	6	..
81	..	..	..	5	24	2	..
82	..	..	..	..	26	5	..
1883	..	..	..	..	29	2	..
Sums	..	..	..	15	403	47	..

September.

1869	..	..	..	4	25	1	..
70	..	..	..	2	28	..	..
71	..	..	..	14	14	2	..
72	..	..	..	13	17	..	..
73	..	..	..	9	21	..	..
74	..	..	..	6	24	..	..
75	..	..	..	6	24	..	..
76	..	..	..	11	19	..	..
77	..	..	..	16	14	..	..
78	..	..	..	20	7	3	..
79	..	..	..	6	24	..	..
80	..	..	..	2	24	4	..
81	..	..	..	4	26	..	..
82	..	..	..	6	24	..	..
1883	..	..	..	7	23	..	..
Sums	..	..	..	126	314	10	..

Table IV—*continued.*

ABERDEEN—*continued.*

October.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	7	13	10	1	..
70	..	..	..	21	10	..	..
71	..	..	..	12	19	..	..
72	..	..	1	25	5	..	..
73	..	..	5	23	3	..	..
74	..	..	1	25	5	..	..
75	..	..	1	24	6	..	..
76	..	..	2	15	14	..	..
77	..	..	6	16	9	..	..
78	..	..	2	11	18	..	..
79	..	..	4	23	4	..	..
80	..	..	10	19	2	..	..
81	..	..	5	20	6	..	..
82	..	..	1	12	18	..	..
1883	..	..	..	25	6	..	..
Sums	..	..	45	284	135	1	..

November.

1869	..	3	15	9	3	..	..
70	..	..	18	12	..	..	..
71	..	..	19	11	..	..	..
72	..	..	8	20	2	..	..
73	..	..	8	22	..	..	..
74	..	..	14	16	..	..	..
75	..	1	17	11	1	..	..
76	..	1	9	20	..	..	..
77	..	..	10	19	1	..	..
78	..	..	18	12	..	..	..
79	..	1	12	17	..	..	..
80	..	4	10	16	..	..	..
81	..	..	3	23	4	..	..
82	..	..	17	13	..	..	..
1883	..	..	16	12	2	..	..
Sums	..	10	194	233	13	..	..

Table IV—*continued.*

ABERDEEN—*continued.*

December.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	6	19	6	..	..	..
70	..	10	16	5	..	..	..
71	..	3	17	11	..	..	..
72	..	3	12	16	..	..	..
73	..	..	11	20	..	..	..
74	..	12	18	1	..	..	..
75	..	..	16	15	..	..	..
76	..	..	12	19	..	..	..
77	..	4	13	14	..	..	..
78	..	18	11	2	..	..	..
79	..	10	13	8	..	..	..
80	1	8	13	9	..	..	..
81	..	4	15	12	..	..	..
82	2	6	14	9	..	..	..
1883	..	1	20	10	..	..	..
Sums	3	85	220	157	..	..	..

FALMOUTH.

January.

1869	..	..	..	24	7	..	..
70	..	..	8	21	2	..	..
71	..	1	16	14	..	..	..
72	..	..	..	27	4	..	..
73	..	..	4	23	4	..	..
74	..	..	..	27	4	..	..
75	..	..	..	18	13	..	..
76	..	2	9	19	1	..	..
77	..	..	..	25	6	..	..
78	..	..	5	24	2	..	..
79	..	5	12	14	..	..	..
80	..	..	13	17	1	..	..
81	..	8	10	13	..	..	..
82	..	..	2	24	5	..	..
1883	..	..	..	26	5	..	..
Sums	..	16	79	316	54	..	..

Table IV—*continued.*

FALMOUTH—*continued.*

February.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	17	11	..	..
70	..	4	8	16	..	..	..
71	..	..	1	23	4	..	..
72	..	..	..	25	4	..	..
73	..	..	11	17	..	..	..
74	..	..	1	27	..	..	..
75	..	1	8	17	2	..	..
76	..	..	6	17	6	..	..
77	..	..	1	18	9	..	..
78	..	..	2	23	3	..	..
79	..	..	9	18	1	..	..
80	..	..	..	27	2	..	..
81	..	..	8	18	2	..	..
82	..	..	..	24	4	..	..
1883	..	..	..	28	..	..	..
Sums	..	5	55	315	48	..	..

March.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	7	24	..	..	..
70	..	..	6	22	3	..	..
71	..	..	..	28	3	..	..
72	..	..	5	14	12	..	..
73	..	..	3	27	1	..	..
74	..	..	3	25	3	..	..
75	..	..	4	25	2	..	..
76	..	..	8	21	2	..	..
77	..	..	3	25	3	..	..
78	..	..	4	23	4	..	..
79	..	..	6	24	1	..	..
80	..	..	..	28	3	..	..
81	..	..	2	25	4	..	..
82	..	..	..	25	6	..	..
1883	..	..	17	14	..	..	..
Sums	..	..	68	350	47	..	..

Table IV—*continued.*

Falmouth—*continued.*

April.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9
1869	..	..	..	11	19	..	..
70	..	..	..	21	9	..	..
71	..	..	..	13	17	..	..
72	..	..	..	20	10	..	..
73	..	..	..	20	10	..	..
74	..	..	..	14	16	..	..
75	..	..	..	23	7	..	..
76	..	..	1	20	9	..	..
77	..	..	..	23	7	..	..
78	..	..	..	19	11	..	..
79	..	..	4	25	1	..	..
80	..	..	..	23	7	..	..
81	..	..	3	23	4	..	..
82	..	..	..	18	12	..	..
1883	..	..	..	28	2	..	..
Sums	..	..	8	301	141	..	..

May.

1869	..	..	..	8	23	..	..
70	..	..	..	8	23	..	..
71	..	..	..	8	21	2	..
72	..	..	..	16	15	..	..
73	..	..	..	10	21	..	..
74	..	..	..	10	21	..	..
75	..	..	..	..	30	1	..
76	..	..	..	13	18	..	..
77	..	..	..	13	18	..	..
78	..	..	..	..	31	..	..
79	..	..	..	15	16	..	..
80	..	..	..	7	23	1	..
81	..	..	..	5	24	2	..
82	..	..	..	4	27	..	..
1883	..	..	..	11	20	..	..
Sums	..	..	..	128	331	6	..

Table IV—*continued*.

FALMOUTH—*continued*.

June.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	..	26	4	..
70	..	..	..	..	23	7	..
71	..	..	..	..	29	1	..
72	..	..	..	..	27	3	..
73	..	..	..	..	22	8	..
74	..	..	..	..	26	4	..
75	..	..	..	..	28	2	..
76	..	..	..	..	23	7	..
77	..	..	..	..	17	13	..
78	..	..	..	..	22	8	..
79	..	..	..	..	30	..	..
80	..	..	..	..	27	3	..
81	..	..	..	1	25	4	..
82	..	..	..	..	30	..	..
1883	..	..	..	..	27	3	..
Sums	..	..	..	1	382	67	..

July.

1869	..	..	..	..	7	24	..
70	..	..	..	..	2	29	..
71	..	..	..	..	21	10	..
72	..	..	..	..	7	24	..
73	..	..	..	..	12	19	..
74	..	..	..	..	9	22	..
75	..	..	..	..	22	9	..
76	..	..	..	..	6	25	..
77	..	..	..	..	20	11	..
78	..	..	..	..	6	25	..
79	..	..	..	..	28	3	..
80	..	..	..	..	14	17	..
81	..	..	..	..	12	19	..
82	..	..	..	..	24	7	..
1883	..	..	..	..	27	4	..
Sums	..	..	..	..	217	248	..

Table IV—*continued.*

FALMOUTH—*continued.*

August.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	..	13	18	..
70	..	..	..	..	9	22	..
71	..	..	..	..	6	24	1
72	..	..	..	..	14	17	..
73	..	..	..	..	13	18	..
74	..	..	..	..	17	14	..
75	..	..	..	..	6	25	..
76	..	..	..	..	10	21	..
77	..	..	..	..	14	17	..
78	..	..	..	..	1	30	..
79	..	..	..	..	26	5	..
80	..	..	..	..	2	29	..
81	..	..	..	..	24	7	..
82	..	..	..	..	18	13	..
1883	..	..	..	..	16	15	..
Sums	..	..	..	..	189	275	1

September.

1869	..	..	..	..	24	6	..
70	..	..	..	..	25	5	..
71	..	..	..	1	20	9	..
72	..	..	..	3	13	14	..
73	..	..	..	..	28	2	..
74	..	..	..	..	26	4	..
75	..	..	..	..	9	21	..
76	..	..	..	..	27	3	..
77	..	..	..	1	28	1	..
78	..	..	..	..	16	14	..
79	..	..	..	..	30	..	..
80	..	..	..	..	20	10	..
81	..	..	..	..	29	1	..
82	..	..	..	..	29	1	..
1883	..	..	..	..	27	3	..
Sums	..	..	..	5	351	94	..

Table IV—*continued.*

FALMOUTH—*continued.*

October.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	10	20	1	..
70	..	..	..	5	26	..	..
71	..	..	..	1	30	..	..
72	..	..	..	15	16	..	..
73	..	..	..	12	19	..	..
74	..	..	..	2	29	..	..
75	..	..	..	6	24	1	..
76	..	..	..	4	27	..	..
77	..	..	..	3	28	..	..
78	..	..	..	8	21	2	..
79	..	..	..	5	26	..	..
80	..	..	..	15	16	..	..
81	..	..	1	9	21	..	..
82	..	..	..	7	24	..	..
1883	..	..	..	4	27	..	..
Sums	..	..	1	106	354	4	..

November.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	1	16	13	..	..
70	..	..	3	20	7	..	..
71	..	..	4	21	5	..	..
72	..	..	1	20	9	..	..
73	..	..	..	23	7	..	..
74	..	..	..	12	18	..	..
75	..	..	6	11	13	..	..
76	..	..	1	13	16	..	..
77	..	..	..	16	14	..	..
78	..	..	3	27	..	..	..
79	..	..	8	17	5	..	..
80	..	..	3	16	11	..	..
81	..	..	..	8	22	..	..
82	..	..	1	21	8	..	..
1883	..	..	..	22	8	..	..
Sums	..	..	31	263	156	..	..

Table IV—*continued.*

FALMOUTH—*continued.*

December.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	2	8	19	2	..	..
70	..	5	11	13	2	..	..
71	..	..	9	22	..	..	..
72	..	..	1	24	6	..	..
73	..	..	..	28	3	..	..
74	..	..	10	21	..	..	..
75	..	1	9	18	3	..	..
76	..	..	3	16	12	..	..
77	..	..	..	28	3	..	..
78	..	2	17	10	2	..	..
79	..	..	10	20	1	..	..
80	.	..	2	14	15	..	..
81	..	..	5	24	2	..	..
82	..	..	7	16	8	..	..
1883	..	..	4	24	3	..	..
Sums	..	10	96	297	62	..	..

STONYHURST.

January.

1869	..	1	7	23	..	..	..
70	..	5	14	12	..	..	..
71	..	16	13	2	..	..	..
72	..	..	12	19	..	..	..
73	..	4	10	16	1	..	..
74	..	..	9	22	..	..	..
75	..	1	6	23	1	..	..
76	..	4	12	15	..	..	..
77	..	..	12	19	..	..	..
78	..	1	14	15	1	..	..
79	..	23	7	1	..	..	..
80	..	13	10	7	1	..	..
81	4	15	7	5	..	..	..
82	..	..	8	23	..	..	..
1883	..	1	19	10	1	..	..
Sums	4	84	160	212	5	..	..

Table IV—*continued.*

STONYHURST—*continued.*

February.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	5	21	2	..	..
70	..	7	13	8	..	..	..
71	..	2	6	20	..	..	..
72	..	..	3	26	..	..	..
73	..	5	23	..	..	..	..
74	..	7	7	14	..	..	..
75	..	3	18	7	..	..	..
76	..	3	11	15	..	..	..
77	..	2	7	19	..	..	..
78	..	1	12	14	1	..	..
79	..	5	18	5	..	..	..
80	..	..	9	20	..	..	..
81	..	3	21	4	..	..	..
82	..	..	7	21	..	..	..
1883	..	..	10	18	..	..	..
Sums	..	38	170	212	3	..	..

March.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	23	8	..	..	..
70	..	..	16	15	..	..	..
71	..	..	8	18	5	..	..
72	..	1	7	19	4	..	..
73	..	..	18	13	..	..	..
74	..	2	4	25	..	..	..
75	..	..	17	14	..	..	..
76	..	1	18	12	..	..	..
77	..	1	16	14	..	..	..
78	..	1	14	16	..	..	..
79	..	4	10	17	..	..	..
80	..	..	11	20	..	..	..
81	..	3	13	15	..	..	..
82	..	..	4	25	2	..	..
1883	..	6	20	5	..	..	..
Sums	..	19	199	236	11	..	..

Table IV—*continued.*

STONYHURST—*continued.*

April.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	1	19	8	2	..
70	..	..	..	26	4	..	..
71	..	..	2	25	3	..	..
72	..	..	3	22	5	..	..
73	..	..	4	22	4	..	..
74	..	..	..	19	10	1	..
75	..	..	..	21	9	..	..
76	..	..	4	21	5	..	..
77	..	..	3	27	..	..	..
78	..	..	4	18	8	..	..
79	..	..	12	18	..	..	..
80	..	..	..	28	2	..	..
81	..	..	10	18	2	..	..
82	..	..	1	25	4	..	..
1883	..	..	1	29	..	..	..
Sums	..	..	45	338	64	3	..

May.

1869	..	..	1	28	2	..	..
70	..	..	..	11	20	..	..
71	..	..	..	13	16	2	..
72	..	..	..	22	9	..	..
73	..	..	..	20	11	..	..
74	..	..	..	19	12	..	..
75	..	..	..	23	8	..	..
76	..	..	..	19	12	..	..
77	..	..	3	21	7	..	..
78	..	..	..	8	23	..	..
79	..	..	3	19	9	..	..
80	..	..	..	19	12	..	..
81	..	..	..	11	15	5	..
82	..	..	..	13	18	..	..
1883	..	..	1	13	17	..	..
Sums	..	..	8	259	191	7	..

Table IV—*continued*.

STONYHURST—*continued*.

June.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	2	24	4	..
70	..	..	..	..	23	7	..
71	..	..	..	6	21	3	..
72	..	..	..	2	19	9	..
73	..	..	..	..	26	4	..
74	..	..	..	..	29	1	..
75	..	..	..	..	27	3	..
76	..	..	..	..	24	6	..
77	..	..	..	..	23	7	..
78	..	..	..	1	22	5	2
79	..	..	..	4	26	..	..
80	..	..	..	4	22	4	..
81	..	..	..	5	21	4	..
82	..	..	..	3	26	1	..
1883	..	..	..	1	26	3	..
Sums	..	..	..	28	359	61	2

July.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	..	12	18	1
70	..	..	..	..	14	17	..
71	..	..	..	..	27	4	..
72	..	..	..	..	12	18	1
73	..	..	..	..	18	11	2
74	..	..	..	..	16	15	..
75	..	..	..	..	19	12	..
76	..	..	..	..	11	18	2
77	..	..	..	..	28	3	..
78	..	..	..	..	17	12	2
79	..	..	..	..	28	3	..
80	..	..	..	..	27	4	..
81	..	..	..	..	22	9	..
82	..	..	..	..	26	5	..
1883	..	..	..	1	24	6	..
Sums	..	..	..	1	301	155	8

Table IV—*continued.*

STONYHURST—*continued.*

August.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	2	22	5	2
70	..	..	..	..	17	14	..
71	..	..	..	..	11	20	..
72	..	..	..	..	22	9	..
73	..	..	..	..	23	8	..
74	..	..	..	..	27	4	..
75	..	..	..	..	15	16	..
76	..	..	..	1	16	14	..
77	..	..	..	..	20	11	..
78	..	..	..	..	17	14	..
79	..	..	..	..	27	4	..
80	..	..	..	..	16	15	..
81	..	..	..	..	29	2	..
82	..	..	..	..	23	8	..
1883	..	..	..	..	27	4	..
Sums	..	..	..	3	312	148	2

September.

1869	..	..	..	..	25	5	..
70	..	..	..	1	29	..	..
71	..	..	..	12	17	1	..
72	..	..	..	9	13	8	..
73	..	..	..	6	24	..	..
74	..	..	..	1	28	1	..
75	..	..	..	..	23	7	..
76	..	..	..	3	26	1	..
77	..	..	..	9	21	..	..
78	..	..	..	5	20	5	..
79	..	..	..	5	25	..	..
80	..	..	..	1	23	6	..
81	..	..	..	..	30	..	..
82	..	..	..	6	24	..	..
1883	..	..	..	1	28	1	..
Sums	..	..	..	59	356	35	..

Table IV—*continued*.

Stonyhurst—*continued*.

October.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	4	11	14	2	..
70	..	..	..	24	7	..	..
71	..	..	..	..	22	9	..
72	..	..	..	26	5	..	..
73	..	..	6	16	8	1	..
74	..	..	..	21	10	..	..
75	..	..	..	21	10	..	..
76	..	..	1	13	16	1	..
77	..	..	2	22	7	..	..
78	..	..	1	11	17	2	..
79	..	..	2	21	8	..	..
80	..	..	10	20	1	..	..
81	..	..	5	22	4	..	..
82	..	..	..	17	14	..	..
1883	..	..	1	19	11	..	..
Sums	..	..	32	264	154	15	..

November.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	1	10	15	4	..	..
70	..	..	15	15	..	..	..
71	..	2	18	10	..	..	..
72	..	..	9	18	3	..	..
73	..	..	8	21	1	..	..
74	..	..	11	14	5	..	..
75	..	..	16	10	4	..	..
76	..	..	12	15	3	..	..
77	..	..	4	24	2	..	..
78	..	2	22	6	..	..	..
79	..	2	15	12	1	..	..
80	..	3	12	14	1	..	..
81	..	..	3	18	9	..	..
82	..	..	15	13	2	..	..
1883	..	..	7	22	1	..	..
Sums	..	10	177	227	36	..	..

Table IV—*continued.*

Stonyhurst—*continued.*

December.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	6	18	7	..	..	..
70	..	13	13	5	..	..	..
71	..	3	12	16	..	..	..
72	..	3	12	16	..	..	..
73	..	2	6	23	..	..	..
74	1	15	11	4	..	..	..
75	..	3	11	17	..	..	..
76	..	2	5	24	..	..	..
77	..	1	8	22	..	..	..
78	1	17	11	2	..	..	..
79	..	12	14	5	..	..	..
80	..	2	15	14	..	..	..
81	..	4	15	12	..	..	..
82	..	7	13	11	..	..	..
1883	..	1	13	17	..	..	..
Sums	2	91	177	195	..	..	..

Kew.

January.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	2	6	22	1	..	..
70	..	5	11	15	..	..	..
71	..	10	19	2	..	..	..
72	..	..	7	24	..	..	..
73	..	..	12	16	3	..	..
74	..	..	9	21	1	..	..
75	..	1	4	24	2	..	..
76	..	7	14	9	1	..	..
77	..	..	10	20	1	..	..
78	..	1	15	13	2	..	..
79	..	16	12	3	..	..	..
80	..	11	17	2	1	..	..
81	2	12	11	6	..	..	..
82	..	1	10	20	..	..	..
1883	..	..	12	18	1	..	..
Sums	2	66	169	215	13	..	..

Table IV—*continued.*

Kew—*continued.*

February.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	2	20	6	..	..
70	..	7	11	9	1	..	..
71	..	1	3	23	1	..	..
72	..	..	1	26	2	..	..
73	..	4	20	4	..	..	..
74	..	5	9	14	..	..	..
75	..	5	19	4	..	..	..
76	..	4	8	13	4	..	..
77	..	1	4	20	3	..	..
78	..	1	9	16	2	..	..
79	..	3	14	10	1	..	..
80	..	1	10	17	1	..	..
81	..	1	19	8	..	..	..
82	..	..	9	16	3	..	..
1883	..	..	5	23	..	..	..
Sums	..	33	143	223	24	..	..

March.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	21	10	..	..	..
70	..	1	17	9	4	..	..
71	..	..	5	21	5	..	..
72	..	..	9	14	8	..	..
73	..	..	9	21	1	..	..
74	..	2	3	20	6	..	..
75	..	..	16	13	2	..	..
76	..	1	12	16	2	..	..
77	..	..	15	16	..	..	..
78	..	..	14	13	4	..	..
79	..	1	11	19	..	..	..
80	..	..	4	23	4	..	..
81	..	1	12	13	5	..	..
82	..	..	3	23	5	..	..
1883	..	4	21	6	..	..	..
Sums	..	10	172	237	46	..	..

Table IV—*continued.*

Kew—*continued.*

April.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	1	12	15	2	..
70	..	..	3	15	11	1	..
71	..	..	..	15	15	..	..
72	..	..	1	17	12	..	..
73	..	..	3	19	8	..	..
74	..	..	..	15	15	..	..
75	..	..	..	23	7	..	..
76	..	..	4	14	12	..	..
77	..	..	..	24	6	..	..
78	..	..	1	15	14	..	..
79	..	..	4	24	2	..	..
80	..	..	..	19	11	..	..
81	..	..	6	15	9	..	..
82	..	..	..	23	7	..	..
1883	..	..	1	21	8	..	..
Sums	..	..	24	271	152	3	..

May.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	8	23	..	..
70	..	..	..	10	16	5	..
71	..	..	..	14	14	3	..
72	..	..	..	14	16	1	..
73	..	..	..	12	19	..	..
74	..	..	..	18	9	4	..
75	..	..	..	1	27	3	..
76	..	..	..	19	12	..	..
77	..	..	2	10	19	..	..
78	..	..	..	1	27	3	..
79	..	..	1	15	15	..	..
80	..	..	..	14	15	2	..
81	..	..	..	6	21	4	..
82	..	..	..	6	25	..	..
1883	..	..	1	9	17	4	..
Sums	..	..	4	157	275	29	..

Table IV—*continued.*

Kew—*continued.*

June.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	..	25	5	..
70	..	..	..	..	12	16	2
71	..	..	..	4	20	6	..
72	..	..	..	..	18	10	2
73	..	..	..	..	17	13	..
74	..	..	..	1	19	10	..
75	..	..	..	..	15	15	..
76	..	..	..	..	17	12	1
77	..	..	..	..	11	19	..
78	..	..	..	..	19	6	5
79	..	..	..	..	27	3	..
80	..	..	..	1	16	13	..
81	..	..	..	3	10	17	..
82	..	..	..	1	25	4	..
1883	..	..	..	..	9	20	1
Sums	..	..	..	10	260	169	11

July.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	..	5	22	4
70	..	..	..	..	5	20	6
71	..	..	..	..	14	17	..
72	..	..	..	..	5	23	3
73	..	..	..	..	5	23	3
74	..	..	..	..	3	25	3
75	..	..	..	..	19	12	..
76	..	..	..	..	2	24	5
77	..	..	..	..	11	20	..
78	..	..	..	..	5	24	2
79	..	..	..	..	23	8	..
80	..	..	..	..	10	21	..
81	..	..	..	..	8	15	8
82	..	..	..	..	12	19	..
1883	..	..	..	..	17	13	1
Sums	..	..	..	..	144	286	35

Table IV—*continued.*

Kew—*continued.*

August.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	..	15	14	2
70	..	..	..	..	13	18	..
71	..	..	..	..	3	26	2
72	..	..	..	..	13	18	..
73	..	..	..	..	8	23	..
74	..	..	..	..	15	16	..
75	..	..	..	..	9	21	1
76	..	..	..	..	7	19	5
77	..	..	..	..	10	20	1
78	..	..	..	..	4	27	..
79	..	..	..	..	16	15	..
80	..	..	..	..	4	27	..
81	..	..	..	..	20	11	..
82	..	..	..	..	18	13	..
1883	..	..	..	..	10	21	..
Sums	..	..	..	..	165	289	11

September.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	..	..	18	12	..
70	..	..	..	1	25	4	..
71	..	..	..	3	16	13	..
72	..	..	..	5	11	11	1
73	..	..	..	1	27	2	..
74	..	..	..	..	22	8	..
75	..	..	..	..	13	17	..
76	..	..	..	1	25	4	..
77	..	..	..	9	17	4	..
78	..	..	..	4	20	6	..
79	..	..	..	1	29	..	..
80	..	..	..	..	15	14	1
81	..	..	..	1	27	2	..
82	..	..	..	3	24	3	..
1883	..	..	..	1	27	2	..
Sums	..	..	..	30	316	102	2

Table IV—*continued.*

KEW—*continued.*

October.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	3	12	14	2	..
70	..	..	..	12	19	..	..
71	..	..	..	13	18	..	..
72	..	..	1	21	9	..	..
73	..	1	3	14	12	1	..
74	..	..	..	10	21	..	..
75	..	..	..	19	11	1	..
76	..	..	1	11	14	5	..
77	..	..	1	18	11	1	..
78	..	..	2	10	17	2	..
79	..	..	..	18	13	..	..
80	..	..	5	14	12	..	..
81	..	..	5	22	4	..	..
82	..	..	..	13	17	1	..
1883	..	..	..	15	16	..	..
Sums	..	1	21	222	208	13	..

November.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9.	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	..	11	12	7	..	..
70	..	..	13	16	1	..	..
71	..	3	19	8	..	..	..
72	..	..	8	15	7	..	..
73	..	..	2	24	4	..	..
74	..	3	9	15	3	..	..
75	..	1	12	11	6	..	..
76	..	..	9	15	6	..	..
77	..	..	4	18	8	..	..
78	..	..	14	16	..	..	..
79	..	5	11	14	..	..	..
80	..	2	11	12	5	..	..
81	..	..	3	12	15	..	..
82	..	..	11	13	6	..	..
1883	..	..	7	22	1	..	..
Sums	..	14	144	223	69	..	..

Table IV—*continued.*

Kew—*continued.*

December.

Year.	10°—19°·9.	20°—31°·9.	32°—39°·9.	40°—49°·9	50°—59°·9.	60°—69°·9.	70°—79°·9.
1869	..	5	10	15	1	..	..
70	1	13	10	7	..	..	..
71	..	4	10	17	..	..	..
72	..	..	6	24	1	..	..
73	..	5	4	20	2	..	..
74	..	12	14	5	..	..	..
75	..	6	9	15	1	..	..
76	..	..	6	16	9	..	..
77	..	..	11	20	..	..	..
78	..	15	10	4	2	..	..
79	..	15	13	3	..	..	..
80	..	..	8	20	3	..	..
81	..	1	11	19	..	..	..
82	..	5	12	8	6	..	..
1883	..	1	12	17	1	..	..
Sums	1	82	146	210	26	..	..

The figures were then divided by 15 to obtain the mean frequency of the different temperatures per month, and Table V was thus formed, which is precisely similar in its arrangement to the frequency table in Table III.

Table V.—Frequency of Mean Daily Temperatures.

	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
Valencia.												
10°·0—19°·9	..	..	..	..	..	..	..	..	..	..	..	..
20·0—31·9	0·3	0·1	..	..	..	..	..	..	..	..	..	0·2
32·0—39 9	4·0	3·1	2·3	0·1	..	..	..	..	..	..	1·6	5·5
40·0—49·9	22·3	21·7	24·2	16·7	7·2	0·2	..	..	0·4	9·5	18·4	20·1
50·0—59·9	4·4	3·3	4·5	13·0	23·5	27·1	21·6	18·3	25·0	20·8	10·0	5·2
60·0—69·9	..	..	..	0·1	0·3	2 7	9·4	12·6	4·6	0·7	..	..
70·0—79·9	..	..	..	..	..	..	..	0·1	..	..	..	..
Armagh.												
10·0—19·9	..	..	..	..	..	..	..	..	..	..	..	0·1
20·0—31·9	2·8	0·9	0·2	..	..	..	..	..	..	..	0·4	5·4
32·0—39·9	11·7	8·8	12 0	2·0	0·1	..	..	..	..	1·8	10·9	11·3
40·0—49·9	16·0	17·9	16·8	22·1	14·7	2·1	0·1	0·2	5·7	18·5	15·9	13·9
50·0—59·9	0·5	0·6	2·0	5·7	15·9	24·9	23·1	21·6	22·8	10·3	2·8	0·3
60·0—69·9	..	..	..	0·1	0·2	3·0	7·8	9·1	1·4	0·3	..	..
70·0—79·9	..	..	..	..	..	..	..	..	..	..	..	..
Glasgow.												
10·0—19·9	0·2	..	..	..	..	..	..	..	..	..	..	0·3
20·0—31·9	4·0	2·1	1·1	..	..	..	..	..	..	..	1·6	5·5
32·0—39·9	13·2	11·9	14·5	3·5	0·4	..	..	..	..	2·8	12·2	12·2
40·0—49·9	13·5	14·1	15·1	22·5	17·1	2·4	..	0·7	6·7	19·7	14·3	12·9
50·0—59·9	0·1	0·1	0·3	3·9	12·9	25·5	24·3	24·9	22·7	8·4	11·8	0·1
60·0—69·9	..	..	..	..	0·5	2·0	6·5	5·5	0·6	0·1	0·1	..
70 0—79·9	..	..	..	..	..	0·1	0·2	..	..	..	..	..

Table V—*continued.*

ABERDEEN.

	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
10°·0—19·9	0·1	..	..	..	..	..	..	..	..	..	..	0·2
20·0—31·9	3·5	2·2	2·4	0·1	..	..	..	..	..	..	0·7	5·7
32·0—39·9	15·7	13·7	14.8	6·3	1·3	..	..	..	..	3·0	12·9	14·7
40·0—49·9	11·8	12·1	13·9	21·2	20·9	7·1	0·3	1·0	8·4	18·9	15·5	10·5
50·0—59·9	..	0·2	0·5	2·5	8·6	21·6	25·9	26·9	20·9	9·0	0·9	..
60·0—69·9	..	..	..	..	0·1	1·2	4·7	3·1	0·7	0·1	..	..
70·0—79·9	..	..	..	..	..	..	0·1	..	..	..	..	..

FALMOUTH.

	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
10·0—19·9	..	..	..	..	..	..	..	..	..	..	..	..
20·0—31·9	1·1	0·5	..	..	..	..	..	..	..	..	..	0·7
32·0—39·9	5·3	3·7	4·5	0·5	..	..	..	..	..	0·1	2·1	6·4
40·0—49·9	21·1	21·0	23·3	20·1	8·5	0·1	..	..	0·3	7·1	17·5	19·8
50·0—59·9	3·6	3·2	3·1	9·4	22·1	25·5	14·5	12·6	23 4	23·6	10·4	4·1
60·0—69·9	..	..	..	..	0·4	4·5	16·5	18·3	6·3	0·3	..	..
70·0—79·9	..	..	..	..	..	..	..	0·1	..	..	..	..

STONYHURST.

	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
10·0—19·9	0·3	..	..	..	..	..	..	..	..	..	..	0·1
20·0—31·9	5·6	2·5	1·3	..	..	..	..	..	..	..	0·7	6·1
32·0—39·9	10·7	11·3	13·3	3·0	0·5	..	..	..	..	2·1	11·8	11·8
40·0—49·9	14·1	14·1	15·7	22·5	17·3	1 9	0·1	0·2	3·9	17·6	15·1	13·0
50·0—59·9	0·3	0·2	0·7	4·3	12·7	23·9	20·1	20·8	23·7	10·3	2·4	..
60·0—69·9	..	..	..	0·2	0 5	4·1	10·3	9·9	2·3	1·0	..	..
70·0—79 9	..	..	..	..	..	0·1	0·5	0·1	..	..	..	..

Table V—*continued.*

KEW.

	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
10°·0—19°·9	0·1	..	..	..	..	..	..	..	..	..	..	0·1
20·0—31·9	4·4	2·2	0·7	..	..	..	..	..	..	0·1	0·9	5·5
32·0—39·9	11·3	9·5	11·5	1·6	0·3	..	..	..	..	1·4	9·6	9·7
40·0—49·9	14·2	14·8	15·8	18·1	10·5	0·7	..	..	2·0	14·8	14·9	14·0
50·0—59·9	0·9	1·6	3·1	10·1	18·3	17·3	9·6	11·0	21·1	13·8	4·6	1·7
60·0—69·9	..	..	..	0·2	1·9	11·3	19·1	19·3	6·8	0·9	..	..
70·0—79·9	..	..	..	..	..	0·7	2·3	0·7	0·1	..	..	..

It seemed of interest to exhibit these figures graphically, and Plate 9, illustrating them, has been drawn. All the curves are not shown. Those for Valencia and Falmouth agree so closely, except in July and August, that one line will represent both for most of the year. Similarly, the curves for Armagh, Glasgow, and Stonyhurst agree so exactly in every month that one line suffices to represent them.

I have therefore shown on the diagram four curves for all the months, and five for July and August. The curves represent respectively (1) Aberdeen, (2) Kew, (3) Armagh, Glasgow, or Stonyhurst, (4) Valencia or Falmouth, and (5) Falmouth alone, in the two months specified.

In the diagrams the abscissæ represent temperatures and the ordinates the number of days during which those temperatures were experienced.

It will be noticed that the line representing Aberdeen lies generally on the left hand of the other lines, showing that the lower temperatures are most prevalent at that, the most northern station under consideration. In all but the summer months the curves for the two south-western observatories show decided peaks, corresponding to temperatures between 40° and 50° in winter and between 50° and 60° in summer, while at all the other stations the maxima are not so marked.

The difference between Valencia and Falmouth in August is particularly striking, the figures from 40° to 50° and from 50° to 60° being exactly reversed, Falmouth showing 18·3 days of the higher and Valencia of the lower temperature.

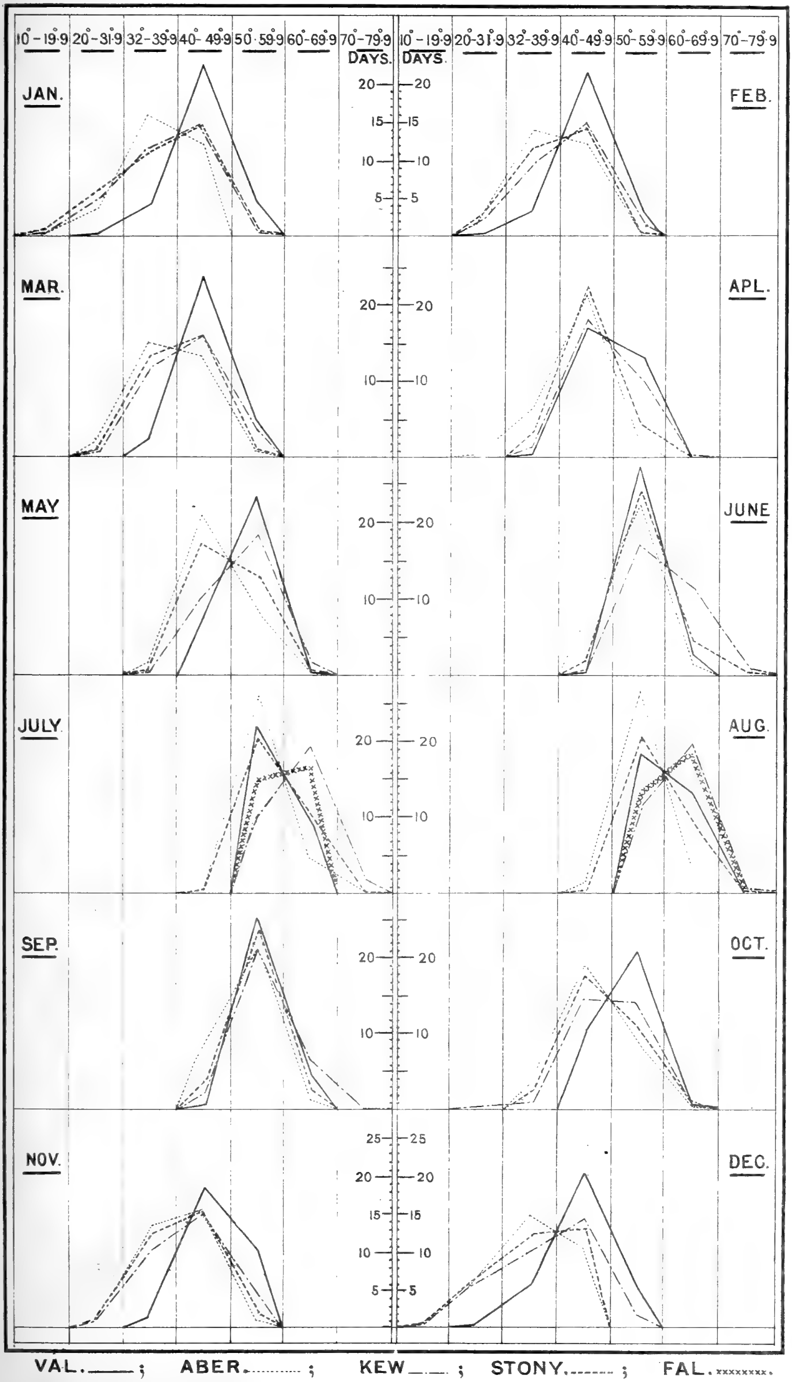
The two months July and August exhibit the chief material difference in climate between the south-west of Ireland and the south of Cornwall—a difference to the advantage of the latter.

We also see from Table V that at both of these south-western stations the mean daily temperature in July never falls below 50°, and never rises above 70°. This amount of equability of temperature is approached, but not quite reached, at several other stations in the same month. At several of the observatories the range of daily mean temperature in winter exceeds forty degrees.

The outcome of the entire enquiry is that, as regards the 15 years under consideration, both (1) the variability of temperature, as defined in the beginning of the paper, and (2) the range of mean temperature, are least at Valencia and Falmouth, the two stations most exposed to the influence of the Atlantic Ocean. Then follows Aberdeen, which, from its close proximity to the sea, enjoys a more equable climate than might have been anticipated from its latitude.

The three stations of Glasgow, Stonyhurst, and Armagh form a third group, and they only differ *inter se* in unimportant particulars.

Diagram showing the distribution of Mean daily Temperature in the British Isles 1869.–1883. inclusive.



West, Newman, lith.

Kew comes last, as the most continental position, with the greatest variability and the highest amount of range. This latter is due to the greater prevalence of high temperatures there than elsewhere.

III. "The Rupture of Steel by Longitudinal Stress." By CHAS. A. CARUS-WILSON. Communicated by Professor G. H. DARWIN, F.R.S. Received March 10, 1890.

Received March 10, 1890.

(Abstract.)

This paper gives an account of experiments made with a view to determining the nature of the resistance that has to be overcome in order to produce rupture in a steel bar by longitudinal stress.

The stress required to produce rupture is in every case computed by dividing the load on the specimen at the moment of breaking by the contracted area at the fracture measured after rupture; this stress is called the "true tensile strength" of the material.

It is well known that any want of uniformity in the distribution of the stress over the ruptured section causes the bar to break at a lower stress than it would if the stress was uniformly distributed. Hence anything that causes want of uniformity is prejudicial; for instance, a groove turned in a cylindrical steel bar will produce want of uniformity, and will consequently be prejudicial, the stress at rupture being lower according as the angle of the groove is more acute. The most favourable condition of test might appear to be that in which a bar of uniform section throughout its length was allowed to draw out freely before breaking, since in this case the stress must be most uniformly distributed.

Experiment, however, shows that the plain bar is not always the strongest. So long as the want of uniformity of stress is considerable, owing to the groove being cut with a very sharp angle, the plain bar is stronger than the grooved bar; but, if the groove be semicircular instead of angular, the grooved bar is considerably stronger than the plain, in spite of the fact that the stress is more uniformly distributed in the latter.

It would seem, then, that we can strengthen a bar over any given section by adding material above and below it, the change in section being gradual; but such an addition of material cannot strengthen the bar if rupture is caused by a certain intensity of tensile stress over the ruptured section; the added material cannot increase the resistance of the ruptured section to direct tensile stress, but it can increase the resistance to the shearing stress.

The resistance of a given section of a steel bar does not, then, depend on its section at right angles to the axis, but on its section at

45° to the axis, for in that direction the shearing stress is a maximum. From this it would seem that the resistance overcome at rupture is the resistance of the steel to shear.

Experiments were made to see whether the resistance of steel to direct shearing bore to its resistance to direct tension the ratio required by the above theory; since the greatest shearing stress is equal to one-half the longitudinal stress, we should expect to find the resistance to direct shearing equal to one-half of the resistance to direct tension.

A series of experiments were made with the result that the ultimate resistance to direct shearing was within, on the average, 3 per cent. of the half of that to direct tension.

The appearance of the fracture of steel bars is next discussed. It would appear that when the stress is uniformly distributed in the neighbourhood of the ruptured section, the fracture is at 45° to the axis, the bar having sheared along that plane which is a plane of least resistance to shear. The tendency to rupture along a plane of shear may be masked by a non-uniform distribution of stress.

Two plates of photographs are added, showing examples of steel bars broken by shearing under longitudinal stress.

IV. "Measurements of the Amount of Oil necessary in order to check the Motions of Camphor upon Water." By Lord Rayleigh, Sec. R.S. Received March 10, 1890.

The motion upon the surface of water of small camphor scrapings, a phenomenon which had puzzled several generations of inquirers, was satisfactorily explained by Van der Mensbrugghe,* as due to the diminished surface-tension of water impregnated with that body. In order that the rotations may be lively, it is imperative, as was well shown by Mr. Tomlinson, that the utmost cleanliness be observed. It is a good plan to submit the internal surface of the vessel to a preliminary treatment with strong sulphuric acid. A touch of the finger is usually sufficient to arrest the movements by communicating to the surface of the water a film of grease. When the surface-tension is thus lowered, the differences due to varying degrees of dissolved camphor are no longer sufficient to produce the effect.

It is evident at once that the quantity of grease required is excessively small, so small that under the ordinary conditions of experiment it would seem likely to elude our methods of measurement. In view, however, of the great interest which attaches to the determination of molecular magnitudes, the matter seemed well worthy of investigation; and I have found that by sufficiently increasing the water

* 'Mémoires Couronnés' (4to) of the Belgian Academy, vol. 34, 1869.

surface the quantities of grease required may be brought easily within the scope of a sensitive balance.

In the present experiments the only grease tried is olive oil. It is desirable that the material which is to be spread out into so thin a film should be insoluble, involatile, and not readily oxidised, requirements which greatly limit the choice.

Passing over some preliminary trials, I will now describe the procedure by which the density of the oil film necessary for the purpose was determined. The water was contained in a sponge-bath of extra size, and was supplied to a small depth by means of an india-rubber pipe in connexion with the tap. The diameter of the circular surface thus obtained was 84 cm. (33″). A short length of fine platinum wire, conveniently shaped, held the oil. After each operation it was cleaned by heating to redness, and counterpoised in the balance. A small quantity of oil was then communicated, and determined by the difference of readings. Two releasements of the beam were tried in each condition of the wire, and the deduced weights of oil appeared usually to be accurate to $\frac{1}{20}$ milligram at least. When all is ready, camphor scrapings are deposited upon the water at two or three places widely removed from one another, and enter at once into vigorous movement. At this stage the oiled extremity of the wire is brought cautiously down so as to touch the water. The oil film advances rapidly across the surface, pushing before it any dust or camphor fragments which it may encounter. The surface of the liquid is then brought into contact with all those parts of the wire upon which oil may be present, so as to ensure the thorough removal of the latter. In two or three cases it was verified by trial that the residual oil was incompetent to stop camphor motions upon a surface including only a few square inches.

The manner in which the results are exhibited will be best explained by giving the details of the calculation for a single case, *e.g.*, the second of December 17. Here 0·81 milligram of oil was found to be very nearly enough to stop the movements. The volume of oil in cubic centimetres is deduced by dividing 0·00081 by the sp. gr., viz., 0·9. The surface over which this volume of oil is spread is

$$\tfrac{1}{4}\pi \times 84^2 \text{ square centimetres};$$

so that the thickness of the oil film, calculated as if its density were the same as in more normal states of aggregation, is

$$\frac{0{\cdot}00081}{0{\cdot}9 \times \frac{1}{4}\pi \times 84^2} = \frac{1{\cdot}63}{10^7} \text{ cm.},$$

or 1·63 micro-millimetres. Other results, obtained as will be seen at considerable intervals of time, are collected in the Table. For conve-

nience of comparison they are arranged, not in order of date, but in order of densities of film.

The sharpest test of the quantity of oil appeared to occur when the motions were nearly, but not quite, stopped. There may be some little uncertainty as to the precise standard indicated by "nearly enough," and it may have varied slightly upon different occasions. But the results are quite distinct, and under the circumstances very accordant. The thickness of oil required to take the life out of the camphor movements lies between one and two millionths of a millimetre, and may be estimated with some precision at 1·6 micro-millimetre. Preliminary results from a water surface of less area are quite in harmony.

For purposes of comparison it will be interesting to note that the

A Sample of Oil, somewhat decolorised by exposure.

Date.	Weight of oil.	Calculated thickness of film.	Effect upon camphor fragments.
Dec. 17...	0·40 mg.	0·81	No distinct effect.
Jan. 11 ..	0·52	1·06	Barely perceptible.
Jan. 14...	0·65	1·32	Not quite enough.
Dec. 20...	0·78	1·58	Nearly enough.
Jan. 11...	0·78	1·58	Just enough.
Dec. 17...	0·81	1·63	Just about enough.
Dec. 18...	0·83	1·68	Nearly enough.
Jan. 22...	0·84	1·70	About enough.
Dec. 18...	0·95	1·92	Just enough.
Dec. 17...	0·99	2·00	All movements very nearly stopped.
Dec. 20...	1·31	2·65	Fully enough.
A fresh Sample.			
Jan. 28...	0·63	1·28	Barely perceptible.
Jan. 28...	1·06	2·14	Just enough.

thickness of the black parts of soap films was found by Messrs. Reinold and Rücker to be 12 micro-millimetres.

An important question presents itself as to how far these water surfaces may be supposed to have been clean to begin with. I believe that all ordinary water surfaces are sensibly contaminated; but the agreement of the results in the Table seems to render it probable that the initial film was not comparable with that purposely contributed. Indeed, the difficulties of the experiments proved to be less than had been expected. Even a twenty-four hours' exposure to the air of the

laboratory* does not usually render a water surface unfit to exhibit the camphor movements.

The thickness of the oil films here investigated is of course much below the range of the forces of cohesion; and thus the tension of the oily surface may be expected to differ from that due to a complete film, and obtained by addition of the tensions of a water-oil surface and of an oil-air surface. The precise determination of the tension of oily surfaces is not an easy matter. A capillary tube is hardly available, as there would be no security that the degree of contamination within the tube was the same as outside. Better results may be obtained from the rise of liquid between two parallel plates. Two such plates of glass, separated at the corners by thin sheet metal, and pressed together near the centre, dipped into the bath. In one experiment of this kind the height of the water when clean was measured by 62. When a small quantity of oil, about sufficient to stop the camphor motions, was communicated to the surface of the water, it spread also over the surface included between the plates, and the height was depressed to 48. Further additions of oil, even in considerable quantity, only depressed the level to 38.

The effect of a small quantity of oleate of soda is much greater. By this agent the height was depressed to 24, which shows that the tension of a surface of soapy water is much less than the combined tensions of a water-oil and of an oil-air surface. According to Quincke, these latter tensions are respectively 2·1 and 3·8, giving by addition 5·9; that of a water-air surface being 8·3. When soapy water is substituted for clean, the last number certainly falls to less than half its value, and therefore much below 5·9.

V. "On the Stability of a Rotating Spheroid of Perfect Liquid." By G. H. BRYAN. Communicated by Professor G. H. DARWIN, F.R.S. Received March 12, 1890.

1. In my communication on "The Waves on a Rotating Liquid Spheroid of Finite Ellipticity,"† I stated that it did not appear possible to give a complete investigation of the criteria of stability of Maclaurin's spheroid when the liquid forming it is free from all traces of viscosity, and equilibrium is liable to be broken by a disturbance of a perfectly general character. As the problem in question appeared to be one of considerable interest, I have, since writing the above paper, put the question to the test of numerical calculation in the case of the simpler types of disturbance, and the results thus obtained have been such as to allow of extension to a perfectly general disturbance.

* In the country.

† 'Phil. Trans.,' A, 1889, p. 187.

On page 210 of my paper, I showed that, if we consider only displacements determined by the spheroidal sectorial harmonic of the second degree, the limit of eccentricity consistent with stability as obtained from my period-equations agrees with that obtained by Riemann* and Basset.† This, of course, it should do, for the type of displacement considered in both investigations is the same, viz., one in which the deformed surface becomes an ellipsoid, but does not remain one of revolution. We thus have a *necessary* condition for stability. But we do not know that it is a *sufficient* condition. In order that this may be so, it is necessary that the critical form thus obtained shall be stable for all *other* types of displacement. The object of the present paper is to show that such is, in fact, the case. Were it otherwise, the limit of eccentricity consistent with stability would have to be determined afresh. It is needless to remark that we are here exclusively considering what Poincaré calls "ordinary" stability, as distinguished from "secular" stability.

2. The symbols employed in the present paper are the same as in my former communication, and the results there proved will be here assumed. For the sake of convenience, the notation and results required for the present work are collected below, and references to the paper in question will be denoted by the letter [E].

The letters α, ζ are used as defined in [E], § 4, (11), (12), viz., if e be the eccentricity of the spheroid—

$$\alpha = \sin^{-1} e,$$

$$\zeta = \cot\alpha = (1-e^2)^{\frac{1}{2}}/e.$$

so that $e = (1+\zeta^2)^{-\frac{1}{2}}$ and ζ is the reciprocal of the quantity denoted by f in Thomson and Tait's 'Natural Philosophy' (vol. 2, § 771).

The functions $p_n(\zeta)$, $q_n(\zeta)$, $t_n^s(\zeta)$, $u_n^s(\zeta)$, are defined as in [E, § 5], equations (24) to (27), viz.:—

$$p_n(\zeta) = \frac{1}{2^n . n!}\left(\frac{d}{d\zeta}\right)^n (\zeta^2+1)^n = (-1)^{\frac{1}{2}n} P_n(\zeta\sqrt{-1}) \quad \ldots\ldots\ldots\ldots \quad (1.)$$

$$t_n^s(\zeta) = (\zeta^2+1)^{\frac{1}{2}s}\left(\frac{d}{d\zeta}\right)^s p_n(\zeta) = \frac{(\zeta^2+1)^{\frac{1}{2}s}}{2^n . n!}\left(\frac{d}{d\zeta}\right)^{n+s} (\zeta^2+1)^n \quad \ldots\ldots \quad (2.),$$

$$q_n(\zeta) = p_n(\zeta)\int_\zeta^\infty \frac{d\zeta}{(\zeta^2+1)\{p_n(\zeta)\}^2} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (3.),$$

$$u_n^s(\zeta) = t_n^s(\zeta)\int_\zeta^\infty \frac{d\zeta}{(\zeta^2+1)\{t_n^s(\zeta)\}^2} = (-1)^s \frac{\overline{n-s}\,!}{\overline{n+s}\,!}(\zeta^2+1)^{\frac{1}{2}s}\left(\frac{d}{d\zeta}\right)^s q_n(\zeta) \quad \ldots\ldots \quad (4.).$$

* 'Göttingen, Abhandlungen,' vol. 9 (1860), ***Mathemat.***, § 9.

† 'Treatise on Hydrodynamics,' vol. 2, p. 124.

The quantities $q_n(\zeta)$ and $u_n^s(\zeta)$ are expressible in a finite form in exactly the same way as the ordinary spherical harmonics of the second kind.* We have, in fact,

$$q_n(\zeta) = (-1)^n\{p_n(\zeta)\cot^{-1}\zeta - \mathrm{R}\} \;\ldots\ldots\ldots\ldots\ldots\ldots \quad (5.),$$

$$u_n^s(\zeta) = (-1)^{n-s}\frac{n-s\,!}{n+s\,!}\left\{t_n^s(\zeta)\cot^{-1}\zeta - \frac{\mathrm{R}'}{(\zeta^2+1)^{\frac{1}{2}s}}\right\} \;\ldots \quad (6.),$$

where R, R′ are known rational algebraic functions of degree $n-1$ and $n+s-1$ respectively in which *all the coefficients are positive.* For example:—

$$p_1(\zeta) = \zeta, \qquad q_1(\zeta) = -\{\zeta\cot^{-1}\zeta - 1\},$$

$$t_1^1(\zeta) = (\zeta^2+1)^{\frac{1}{2}}, \qquad u_1^1(\zeta) = +\frac{1}{1.2}\left\{(\zeta^2+1)^{\frac{1}{2}}\cot^{-1}\zeta - \frac{\zeta}{(\zeta^2+1)^{\frac{1}{2}}}\right\},$$

$$p_2(\zeta) = \tfrac{1}{2}(3\zeta^2+1), \qquad q_2(\zeta) = \tfrac{1}{2}(3\zeta^2+1)\cot^{-1}\zeta - \tfrac{3}{2}\zeta,$$

$$t_2^1(\zeta) = 3\zeta(\zeta^2+1)^{\frac{1}{2}}, \qquad u_2^1(\zeta) = -\frac{1}{2.3}\left\{3\zeta(\zeta^2+1)^{\frac{1}{2}}\cot^{-1}\zeta - \frac{3\zeta^2+2}{(\zeta^2+1)^{\frac{1}{2}}}\right\},$$

$$t_2^2(\zeta) = 3(\zeta^2+1), \qquad u_2^2(\zeta) = +\frac{1}{1.2.3.4}\left\{3(\zeta^2+1)\cot^{-1}\zeta - \frac{3\zeta^3+5\zeta}{\zeta^2+1}\right\},$$

and the corresponding functions of the third, fourth, and fifth degrees can be readily written down from my table in the 'Cambridge Philosophical Proceedings' (*loc. cit.*), by introducing the necessary changes in the signs, and putting "cot $^{-1}$" in place of "coth $^{-1}$."

3. In [E, § 20] I showed that if we consider only displacements of the surface determined by a spheroidal harmonic of degree n and rank s, the condition of secular stability, which, in the present notation, is

$$p_1(\zeta)\cdot q_1(\zeta) - t_n^s(\zeta)\cdot u_n^s(\zeta) > 0 \;\ldots\ldots\ldots\ldots \quad (7.),$$

is a *sufficient*, albeit not a *necessary*, condition for stability when the liquid forming the spheroid is perfect. That the left-hand member of this inequality is essentially positive when $n-s$ is odd has been proved by Poincaré,† and another proof is given below (§ 9).

In [E, § 16] I showed that, in the case of the zonal harmonic displacements of even degree n, the necessary and sufficient condition for ordinary stability is

$$p_1(\zeta)\cdot q_1(\zeta) - p_n(\zeta)\cdot q_n(\zeta) + \frac{4}{n(n+1)}\{t_1^1(\zeta)\cdot u_1^1(\zeta) - p_1(\zeta)\cdot q_1(\zeta)\}$$
$$> 0 \ldots\ldots \quad (8.),$$

* 'Cambridge Philosophical Society Proceedings,' 1888, p. 292.

† 'Acta Mathemat.,' vol. 7, p. 326. Write R_i for $t_n^s(\zeta)$ and S_i for $(2n+1)u_n^s(\zeta)$.

and in [E, § 18] that, for a sectorial harmonic displacement, the necessary and sufficient condition is that

$$p_1(\zeta) \cdot q_1(\zeta) - t_n{}^n(\zeta) \cdot u_n{}^n(\zeta) + \frac{1}{n}\{t_1{}^1(\zeta) \cdot u_1{}^1(\zeta) - p_1(\zeta) \cdot q_1(\zeta)\} > 0 \ . \ (9.);$$

while [E, § 20] if $n = 2$, the last condition leads to exactly the same results as Riemann's and Basset's investigations (as already mentioned), and gives for the critical form

$$1/\zeta = 3{\cdot}1414567,$$

whence $$\zeta = {\cdot}3183236, \text{ approximately} \ \ldots\ldots\ldots \ (10.),$$

and the eccentricity $= \sin 72° \ 20' \ 33'' = {\cdot}9528867$.

4. To prove that the spheroid is "ordinarily" stable until this critical form is reached, we only have to show that conditions (7), (8), or (9) (as the case may be) are satisfied by this value of ζ for every value of n and s. For this purpose I have calculated the numerical values of the products $p_n(\zeta) \cdot q_n(\zeta)$, and $t_n{}^s(\zeta) \cdot u_n{}^s(\zeta)$ for values of n up to 4, and, in the case of the sectorial harmonics ($s = n$), up to $n = 6$ inclusive, taking $\zeta = {\cdot}3183236$. The results calculated to four places of decimals are as follows, the last figure being only approximate :—

n.	s.	$t_n{}^s(\zeta) \cdot u_n{}^s(\zeta)$.	$p_1(\zeta) \cdot q_1(\zeta) - t_n{}^s(\zeta) \cdot u_n{}^s(\zeta)$.
1	0	$p_1(\zeta) \cdot q_1(\zeta) = {\cdot}1904$	—
1	1 (sectorial)	·5360	− ·3456
2	0	·2153	− ·0249
2	2 (sectorial)	·3632	− ·1728
3	1	·1566	+ ·0338
3	3 (sectorial)	·2803	− ·0900
4	0	·1116	+ ·0788
4	2	·1266	+ ·0638
4	4 (sectorial)	·2303	− ·0400
5	5 (sectorial)	·1967	− ·0063
6	6 "	·1743	+ ·0161

From this Table it appears that the expression

$$p_1(\zeta) \cdot q_1(\zeta) - t_n{}^s(\zeta) \cdot u_n{}^s(\zeta)$$

is positive, except when $n = 2$, $s = 0$, and in the case of the first five

sectorial harmonics ($n = s$) in the Table. Thus in every case in which the exact conditions of stability have not been investigated, the *sufficient* condition for secular stability given by the inequality (7) is satisfied. It remains to apply the criteria (8) and (9) to the cases where (7) is not satisfied.

5. First take the case of $n = 2$, $s = 0$.

The fact that $p_1(\zeta) \cdot q_1(\zeta) - p_2(\zeta) \cdot q_2(\zeta)$ is negative in the above Table does not indicate that the spheroid in question is secularly unstable for this particular type of displacement. Its meaning is that the spheroid is more oblate than that form for which the angular velocity is a maximum. As pointed out in Poincaré's memoir,* the disturbed form is here also a spheroid of revolution, and there is no form of "bifurcation" when $p_1(\zeta) \cdot q_1(\zeta) - p_2(\zeta) \cdot q_2(\zeta)$ changes sign.

The condition of "ordinary" stability, from inequality (8) is

$$p_1(\zeta) \cdot q_1(\zeta) - p_2(\zeta) \cdot q_2(\zeta) + \tfrac{2}{3}\{t_1^1(\zeta) \cdot u_1^1(\zeta) - p_1(\zeta) \cdot q_1(\zeta)\} > 0.$$

For the particular value of ζ considered, the left-hand member of this inequality is

$$= -\cdot 0249 + \tfrac{2}{3}(\cdot 3456) = -\cdot 0249 + \cdot 2304 = \cdot 2055,$$

and is positive; therefore (8) is satisfied.

Even in the extreme case when the spheroid becomes flattened out indefinitely, so that ζ approaches the limit zero, we find

$$p_1(\zeta) \cdot q_1(\zeta) - p_2(\zeta) \cdot q_2(\zeta) + \tfrac{2}{3}\{t_1^1(\zeta) \cdot u_1^1(\zeta) - p_1(\zeta) \cdot q_1(\zeta)\}$$
$$= 0 - \frac{1}{4}\frac{\pi}{2} + \frac{2}{3}\left\{\frac{1}{2}\frac{\pi}{2} - 0\right\} = -\frac{\pi}{8} + \frac{\pi}{6} = \frac{\pi}{24},$$

and is positive. This accords with Sir William Thomson's result that Maclaurin's spheroid is essentially stable, however oblate, if it is supposed constrained to remain spheroidal.

6. Next consider the sectorial harmonics. As the displacement corresponding to $n = s = 1$ is a mere shifting of the mass as a whole, and we are dealing with the critical value of ζ for displacements determined by the harmonic of degree and rank 2, there are only three cases to consider. Now since

$$t_1^1(\zeta) \cdot u_1^1(\zeta) - p_1(\zeta) \cdot q_1(\zeta) = \cdot 3456,$$

we find

$$p_1(\zeta) \cdot q_1(\zeta) - t_3^3(\zeta) \cdot u_3^3(\zeta) + \tfrac{1}{3}\{t_1^1(\zeta) \cdot u_1^1(\zeta) - p_1(\zeta) \cdot q_1(\zeta)\}$$
$$= -\cdot 0900 + \cdot 1152 = +\cdot 0252;$$

* 'Acta Mathemat.,' vol. 7, p. 329.

$$p_1(\zeta) \cdot q_1(\zeta) - t_4^4(\zeta) \cdot u_4^4(\zeta) + \tfrac{1}{4}\{t_1^1(\zeta) \cdot u_1^1(\zeta) - p_1(\zeta) \cdot q_1(\zeta)\}$$
$$= -\cdot 0400 + \cdot 0864 = +\cdot 0464;$$

$$p_1(\zeta) \cdot q_1(\zeta) - t_5^5(\zeta) \cdot u_5^5(\zeta) + \tfrac{1}{5}\{t_1^1(\zeta) \cdot u_1^1(\zeta) - p_1(\zeta) \cdot q_1(\zeta)\}$$
$$= -\cdot 0063 + \cdot 0691 = +\cdot 0628.$$

The values of these expressions are all positive; therefore condition (9) is satisfied in each case, and the spheroid is "ordinarily" stable for the corresponding types of displacement.

It is therefore stable for *all* types of displacement considered in the foregoing table, except that for which it is, by hypothesis, "critical."

7. On examining the values of $t_n^s(\zeta) \cdot u_n^s(\zeta)$ given in the Table, it appears probable that as we proceed to harmonics of higher degrees this product diminishes in value, and that condition (7) is satisfied universally in all the cases not considered above. That such is actually the case we now proceed to demonstrate. The results are a slight extension of those obtained in § 10 of Poincaré's paper, the method here employed being very similar.

8. Consider the expression—

$$t_m^r(\zeta_0) \cdot u_m^r(\zeta_0) - t_n^s(\zeta_0) \cdot u_n^s(\zeta_0),$$

and let us examine under what circumstances it is essentially positive. From formula (4) we have

$$t_m^r(\zeta_0) \cdot u_m^r(\zeta_0) - t_n^s(\zeta_0) \cdot u_n^s(\zeta_0)$$
$$= \{t_m^r(\zeta_0)\}^2 \int_{\zeta_0}^{\infty} \frac{d\zeta}{(\zeta^2+1)\{t_m^r(\zeta)\}^2} - \{t_n^s(\zeta_0)\}^2 \int_{\zeta_0}^{\infty} \frac{d\zeta}{(\zeta^2+1)\{t_n^s(\zeta)\}^2}$$
$$= \int_{\zeta_0}^{\infty} \frac{d\zeta}{\zeta^2+1} \left\{ \left(\frac{t_m^r(\zeta_0)}{t_m^r(\zeta)}\right)^2 - \left(\frac{t_n^s(\zeta_0)}{t_n^s(\zeta)}\right)^2 \right\}.$$

This will be essentially positive if the quantity to be integrated is always positive, that is, if for all values of ζ lying between ζ_0 and ∞,

$$\left(\frac{t_m^r(\zeta_0)}{t_m^r(\zeta)}\right)^2 - \left(\frac{t_n^s(\zeta_0)}{t_n^s(\zeta)}\right)^2 > 0,$$

or

$$\frac{t_n^s(\zeta)}{t_m^r(\zeta)} > \frac{t_n^s(\zeta_0)}{t_m^r(\zeta_0)},$$

where $\zeta > \zeta_0$,

which will be the case if $t_n^s(\zeta)/t_m^r(\zeta)$ increases with ζ.

9. The result proved by Poincaré, and assumed in the preceding investigations, namely, that if $n-s$ be *odd*—

$$p_1(\zeta) \cdot q_1(\zeta) - t_n^s(\zeta) \cdot u_n^s(\zeta)$$

is essentially positive, follows at once. For, as just proved, this will be the case if $t_n^s(\zeta)/p_1(\zeta)$, that is, $t_n^s(\zeta)/\zeta$, increases with ζ. Now, from formula (2) it is evident that, $n-s$ being odd, $t_n^s(\zeta)$ is divisible by ζ, and the quotient will be $(\zeta^2+1)^{\frac{1}{2}s} \times$ a rational algebraic function of ζ^2 in which all the terms are positive. This quotient evidently increases with ζ, which proves the result.

10. Let us now revert to the original question, but suppose in addition that both $m-r$ and $n-s$ are even. We have just shown that

$$t_m^r(\zeta_0) \cdot u_m^r(\zeta_0) > t_n^s(\zeta_0) \cdot u_n^s(\zeta_0),$$

provided that $t_n^s(\zeta)/t_m^r(\zeta)$ increases with ζ.

This will be the case if

$$\frac{d}{d\zeta} \frac{t_n^s(\zeta)}{t_m^r(\zeta)} > 0,$$

or

$$t_m^r(\zeta) \frac{dt_n^s(\zeta)}{d\zeta} - t_n^s(\zeta) \frac{dt_m^r(\zeta)}{d\zeta} > 0;$$

or multiplying by ζ^2+1,

$$(\zeta^2+1) t_m^r(\zeta) \frac{dt_n^s(\zeta)}{d\zeta} - (\zeta^2+1) t_n^s(\zeta) \frac{dt_m^r(\zeta)}{d\zeta} > 0.$$

Since $m-r$ and $n-s$ are both even, it readily appears that $dt_n^s(\zeta)/d\zeta$ and $dt_m^r(\zeta)/d\zeta$ vanish when $\zeta = 0$.

Hence the left-hand side of the last inequality will be positive when $\zeta > 0$ if it increases with ζ; this condition gives on again differentiating—

$$t_m^r(\zeta) \frac{d}{d\zeta} \left\{ (\zeta^2+1) \frac{dt_n^s(\zeta)}{d\zeta} \right\} - t_n^s(\zeta) \frac{d}{d\zeta} \left\{ (\zeta^2+1) \frac{dt_m^r(\zeta)}{d\zeta} \right\} > 0.$$

Now $t_m^r(\zeta)$ and $t_n^s(\zeta)$ satisfy the differential equations

$$\frac{d}{d\zeta} \left\{ (\zeta^2+1) \frac{dt_n^s(\zeta)}{d\zeta} \right\} = \left\{ n(n+1) - \frac{s^2}{\zeta^2+1} \right\} t_n^s(\zeta),$$

$$\frac{d}{d\zeta} \left\{ (\zeta^2+1) \frac{dt_m^r(\zeta)}{d\zeta} \right\} = \left\{ m(m+1) - \frac{r^2}{\zeta^2+1} \right\} t_m^r(\zeta);$$

therefore we get $\left\{ n(n+1) - m(m+1) - \frac{s^2-r^2}{\zeta^2+1} \right\} t_m^r(\zeta) . t_n^s(\zeta) > 0$, or, since $t_n^s(\zeta)$ and $t_m^r(\zeta)$ are essentially positive,

$$(n-m)(n+m+1)-\frac{s^2-r^2}{\zeta^2+1} > 0 \quad \ldots\ldots\ldots\ldots (11).$$

Writing ζ for ζ_0, we see that inequality (11) is a sufficient condition that the expression—

$$t_m{}^r(\zeta) \cdot u_m{}^r(\zeta) - t_n{}^s(\zeta) \cdot u_n{}^s(\zeta)$$

may be positive in the case of $m-r$ and $n-s$ both even, provided that (11) is satisfied for all values of ζ between 0 and ∞.

11. We have to consider two cases:—

I. Suppose $n = m$. Then condition (11) will be satisfied, for all values of ζ, provided that $r > s$. Therefore $t_n{}^r(\zeta) \cdot u_n{}^r(\zeta)$ is always $> t_n{}^s(\zeta) \cdot u_n{}^s(\zeta)$ whatever be the value of ζ, provided that $r > s$. In other words, for given values of n, ζ, the product $t_n{}^s(\zeta) \cdot u_n{}^s(\zeta)$ increases as s increases, and is greatest when $s = n$ (corresponding to the sectorial harmonics).

II. Suppose $n-s = m-r$ and, therefore, $n-m = s-r$. Condition (11) may be written—

$$(n-m)\left\{n+m+1-\frac{s+r}{\zeta^2+1}\right\} > 0.$$

The first factor is positive provided that $n > m$. The second is necessarily positive, for r, s are not greater respectively than m, n; therefore $n+m+1 > s+r$, and therefore, *a fortiori*,

$$n+m+1 > \frac{s+r}{\zeta^2+1}$$

for all values of ζ. Hence, putting $s = n-2k$, and therefore $r = m-2k$, we have—

$$t_m{}^{m-2k}(\zeta) \cdot u_m{}^{m-2k}(\zeta) > t_n{}^{n-2k}(\zeta) \cdot u_n{}^{n-2k}(\zeta),$$

provided that m is $< n$. Therefore the product $t_n{}^{n-2k}(\zeta) \cdot u_n{}^{n-2k}(\zeta)$ decreases for any given value whatever of ζ as n increases. In particular, $t_n{}^n(\zeta) \cdot u_n{}^n(\zeta)$ decreases as the number n is increased.

12. Let us now apply these results to the spheroid under consideration. From the results of Case II,

$$t_5{}^3(\zeta) \cdot u_5{}^3(\zeta) < t_4{}^2(\zeta) \cdot u_4{}^2(\zeta),$$

$$t_5{}^1(\zeta) \cdot u_5{}^1(\zeta) < t_4{}^0(\zeta) \cdot u_4{}^0(\zeta) \ (i.e., \ p_4(\zeta) \cdot q_4(\zeta));$$

and from the Table, $t_4{}^2(\zeta) \cdot u_4{}^2(\zeta)$ and $p_4(\zeta) \cdot q_4(\zeta)$ are each less than $p_1(\zeta) \cdot q_1(\zeta)$ for this particular value of ζ. Therefore, *a fortiori*,

$$\left.\begin{aligned} &p_1(\zeta) \cdot q_1(\zeta) - t_5{}^3(\zeta) \cdot u_5{}^3(\zeta) > 0 \\ \text{and} \quad &p_1(\zeta) \cdot q_1(\zeta) - t_5{}^1(\zeta) \cdot u_5{}^1(\zeta) > 0 \end{aligned}\right\} \text{ where } \zeta = \cdot 3183\ldots,$$

and the spheroid is stable for harmonic displacements of the degree 5.

From the results of Case II we also have, if n be greater than 6,

$$t_n^n(\zeta) \cdot u_n^n(\zeta) < t_6^6(\zeta) \cdot u_6^6(\zeta),$$

and from the Table

$$t_6^6(\zeta) \cdot u_6^6(\zeta) < p_1(\zeta) \cdot q_1(\zeta);$$

therefore, *a fortiori*,

$$t_n^n(\zeta) \cdot u_n^n(\zeta) < p_1(\zeta) \cdot q_1(\zeta), \text{ if } n > 6.$$

Moreover, by Case I,

$$t_n^s(\zeta) \cdot u_n^s(\zeta) < t_n^n(\zeta) \cdot u_n^n(\zeta);$$

therefore, *a fortiori*,

$$t_n^s(\zeta) \cdot u_n^s(\zeta) < p_1(\zeta) \cdot q_1(\zeta),$$

or

$$p_1(\zeta) \cdot q_1(\zeta) - t_n^s(\zeta) \cdot u_n^s(\zeta) > 0,$$

where $\zeta = \cdot 3183\ldots$, and n is equal to or greater than 6.

Thus the sufficient condition of secular stability is satisfied for all types of displacement, with the exceptions already considered in which the "ordinary" conditions of stability have been proved to hold good. Hence the results of the present paper prove conclusively that *Maclaurin's spheroid, if formed of perfectly inviscid liquid, will be absolutely stable if its eccentricity be less than* 0·9528867. *If the eccentricity exceed this limit, the spheroidal form will become unstable, and the liquid will assume the form of an ellipsoid.*

13. The state of steady motion which then ensues is intermediate between the forms known as Jacobi's and Dedekind's ellipsoids. The "spin" of the liquid will be everywhere constant and equal, say, to ω, and the form of the liquid free surface will be an ellipsoid, whose principal axes rotate about the least axis with angular velocity $\frac{1}{2}\omega$. That this is initially the case is in accordance with the results of [E, §§ 14, 18], supposing that the roots of the period-equation become complex, for their *real* part will indicate that the disturbance travels round with angular velocity $\frac{1}{2}\omega$. It is unnecessary to discuss this point at greater length here.

It is also to be noted that the results of the present paper quite preclude the possibility, under ordinary circumstances, of Maclaurin's spheroid ever passing into the form of one or more rings of rotating liquid. This might probably take place if we imagined the liquid surface constrained to remain a figure of revolution. But such hypothetical circumstances are devoid of interest, and, since it appears from the results of the present analysis that, when we consider displace-

ments determined by harmonics of any even degree (n), the "coefficient of stability" for the displacement symmetrical about the axis is the *last* to change sign, it is clear that hardly any less general constraint would suffice to produce such a result.

VI. "A Determination of "v," the Ratio of the Electromagnetic Unit of Electricity to the Electrostatic Unit." By J. J. THOMSON, M.A., F.R.S., Cavendish Professor of Experimental Physics, Cambridge, and G. F. C. SEARLE, B.A., Peterhouse, Demonstrator in the Cavendish Laboratory, Cambridge. Received March 12, 1890.

(Abstract.)

The experiments made by one of us in 1883 having given a value for "v" considerably smaller than those found in several recent researches on this subject, it was thought desirable to repeat the experiments. The method used in 1883 was to find both the electrostatic and the electromagnetic measures of the capacity of a condenser, the electrostatic measure being calculated from the dimensions of the condenser, and the electromagnetic measure by determining a resistance which would produce the same effect as that produced by repeated charging of the condenser when placed in one arm of a Wheatstone's bridge. In the experiments in 1883 the condenser used in determining the electromagnetic measure was not the same as that for which the electrostatic capacity had been calculated, but one without a guard ring, the equality of the capacity of this condenser and the guard ring condenser being tested by the method given in Maxwell's 'Electricity and Magnetism,' vol. 1, p. 324.

In repeating the experiments we adopted at first the same method as before, using, however, a key of different design for testing the equality of the condensers by Maxwell's method. We got very consistent results, practically identical with those obtained in 1883. We may mention here, since it has been suggested that the capacity of the leads might explain the low value of "v" obtained previously, that the leads are allowed for by the way the comparison between the two condensers is made, for the same leads are used in the determination of the electromagnetic measure of the capacity of the auxiliary condenser and in the comparison of the capacity of this condenser with the one with the guard ring, and the capacity of the auxiliary condenser is adjusted until its capacity, plus that of the leads, equals the capacity of the guard ring condenser; and in the electromagnetic measurements it is the capacity of the auxiliary condenser, plus that of its leads, which is found.

As the use of the auxiliary condenser introduces additional sources of error, we endeavoured to determine the electromagnetic measure of the capacity of the guard ring directly, using a complicated commutator, which worked both the guard ring and condenser. The first commutator we used was one where the contacts were made by platinum styles attached to a tuning fork; the results obtained with this were not so regular as we desired, so we replaced the tuning fork commutator by a rotating one driven by a water motor. A stroboscopic arrangement was attached to the commutator, which enabled its speed to be measured and kept constant. With this arrangement, which worked perfectly, we got values for the electromagnetic measure of the capacity of the condenser distinctly less than those obtained by the old method. We then endeavoured to find out the reason for this difference, and after a good deal of trouble discovered that in the experiments by which the equality of the capacities of the guard ring and auxiliary condensers were tested the guard ring did not produce its full effect. When the guard ring of the standard condenser was removed and the capacity of the auxiliary condenser made the same, the two methods gave identical results, but the effect produced by adding the guard ring was less in the old method than in the new. We found by calculation that the effect produced by the addition of the guard ring in the old method was distinctly too small, while in the new the observed and calculated effects agreed well together. As the new method was working perfectly satisfactorily, and as it possesses great advantages over the old one, inasmuch as we get rid entirely of the auxiliary condenser, and, since the commutator is a rotating one, its speed can be altered with much greater ease and accuracy than can be done with a tuning fork, we discarded the old method and adopted the new one.

The following are the results obtained by this method:—

Electrostatic measure of the capacity, 397·991.

Electromagnetic Measure.

First Set of Experiments.

Number of times the condenser is charged per second.	Capacity × 10^{21}.
64	443·427
32	443·571
48	443·523
80	443·459
64	443·298
55	443·478
42	443·443

Mean, 443·457.

Second Set.

64	443·043
48	443·097
32	443·378
80	442·950
64	443·686
55	443·766
48	443·378
32	443 646
16	443·672
80	443·163

Mean, 443 377.

Third Set.

64	443·369
32	443·257
48	433·770
80	443·530
55	443·835
64	443·401

Mean, 443·527.

The mean of all the observations = $443·454 \times 10^{-21}$.

The means of the observations for different speeds are given in the following table:—

Number of times the condenser is charged per second.	Capacity $\times 10^{21}$.
80	443·275
64	443·370
55	443·693
48	443·442
42	443·443
32	443·463
16	443·672

These agree very well together, the greatest difference being about one part in 1,000.

Taking $443·454 \times 10^{-21}$ as the electromagnetic measure of the capacity, the value of "v" is 299·58.

VII. "On the progressive Paralysis of the different Classes of Nerve Cells in the Superior Cervical Ganglion." By J. N. LANGLEY, F.R.S., Fellow and Lecturer of Trinity College, and W. LEE DICKINSON, M.R.C.P., Caius College, Cambridge. Received March 15, 1890.

It is well known that by stimulating the sympathetic nerve in the neck the following effects can be produced:—(1) Retraction of the nictitating membrane; (2) protrusion of the eyeball and opening of the eye; (3) turning the eye, if previous to stimulation the optic axis is directed nasally, so that the optic axis is directed straight forwards, or it may be forwards and a little outwards; (4) dilation of the pupil; (5) constriction of the small arteries of the ear, conjunctiva, and of various other parts of the head; (6) in the dog, dilation of the small arteries of the gums, lips, and of some other parts of the head; (7) secretion of saliva.

We have shown that the superior cervical ganglion contains nerve cells, interpolated in the course of the nerve fibres concerned in producing all the above effects, and, further, that these nerve cells are readily paralysed by nicotin.* In this paper we consider the question whether the nerve cells are paralysed simultaneously or in a definite order. That different classes of nerve cells are in some cases unequally affected by nicotin has been already shown by one of us (L., *op. cit.*), in so far that in the cat the secretory nerve cells on the course of the cervical sympathetic are more readily paralysed than the secretory nerve cells on the course of the chorda tympani; that in the dog the reverse is the case; and, lastly, that the nerve cells on the course of the secretory fibres of the chorda tympani are paralysed before those on the course of its vaso-dilator fibres.

The method employed has been to inject nicotin into a vein, (*a*) in successive doses, the first dose being rather less than that required to produce complete paralysis of the cervical sympathetic, and to note the order in which the effects normally produced by stimulating the sympathetic disappear; (*b*) in quantities sufficient to cause complete paralysis of the cervical sympathetic, and by stimulating it at short intervals to note the order of recovery of the normal effects of such stimulation.

Of course, by injecting the alkaloid into the blood, the peripheral nerve endings, as well as the nerve cells of the superior cervical ganglion, are exposed to its action; but since, as we have shown

* Langley and Dickinson, 'Roy. Soc. Proc.,' vol. 46, 1889, p. 423; Langley, 'Journal of Physiology,' vol. 11, 1890, p. 146.

(*op. cit.*), even large doses of nicotin* do not prevent the normal sympathetic effect from being obtained on stimulating peripherally of the superior cervical ganglion; any absence of the normal effect of stimulating the sympathetic in the neck which may be caused by a small dose of nicotin must be due to the action of the alkaloid on the nerve cells of the ganglion.

The method of injecting nicotin into a blood vessel is preferable to that of applying dilute nicotin to the ganglion itself (although this has the advantage of limiting the effect to the ganglion), because of the difficulty of applying the nicotin in such a way as to make certain that equal amounts reach all the nerve cells; by the latter method it might be possible for the external cells of the ganglion to be paralysed and the internal cells to have escaped paralysis.

In the course of our experiments we have naturally had frequent occasion to observe the effect of stimulating the sympathetic upon the blood supply of the lips and gums. It will be convenient to discuss this action before proceeding to the more immediate object of our experiments.

Effect of Stimulating the Sympathetic upon the Bucco-labial Region.—The discovery in the sympathetic of the dog of vaso-dilator fibres for the lips, gums, and of some other parts of the head is due to Dastre and Morat.† The whole region in which dilation is produced they call the bucco-facial region; this includes the mucous membrane of the nose, hard palate, of the gums, lips, and the neighbouring cutaneous regions. On the other hand, the same stimulus produces constriction of the small arteries in the epiglottis, tonsils, and soft palate. Bochefontaine and Vulpian‡ observed, that sometimes the dilation was preceded by a constriction. Dastre and Morat§ later found a similar constriction; they state that it occurs only with a certain strength of current, which is a little less than that required to produce primary dilation, so that, when electric shocks cause constriction before the dilation, no effect is produced if the shocks are made a little weaker, and primary dilation is produced if they are made a little stronger.

In our experiments, the variation in the strength of the shocks

* In a recent experiment upon a rabbit 1450 mgm. of nicotin were injected into a vein without causing the heart to stop. Stimulation of the filament running from the superior cervical ganglion to the internal carotid, *i.e.*, stimulation of the sympathetic peripherally of the ganglion, still caused dilation of the pupil. As the experiments in this paper show, 5 to 10 mgm. of nicotin are sufficient to prevent stimulation of the sympathetic in the neck, *i.e.*, of the sympathetic centrally of the ganglion, from producing any effect on the pupil.

† Dastre and Morat, 'Comptes Rendus de l'Acad. des Sciences,' vol. 91, 1880, pp. 393 and 441.

‡ Bochefontaine and Vulpian, 'Soc. de Biologie,' 1880, p. 319.

§ Dastre and Morat, 'Le Système Nerveux Vaso-Moteur' (Paris), 1884, p. 180.

capable of producing primary contraction was much greater than that given by Dastre and Morat. On gradually increasing the strength of the shocks we find with minimal shocks a slight paling of the lips and gums, which only slowly disappears, so that the original pinkish state of the mucous membrane is not regained for one to two minutes after the end of the stimulation. As the shocks are gradually increased in strength the paling becomes more marked, and the after-paling of less duration; with a certain increase in the strength of shocks, the paling continues for a short time after the stimulation, and then gives way to a slight flushing; with further increase, the duration of the after-paling diminishes and the after-flush increases, so that soon the pallor gives way, even during the continuance of the stimulation, to intense flushing. After this, a slight further increase in the strength of the shocks causes primary flushing. Marked flushing is first produced in the anterior part of the lips and gums; a stronger current is required to produce it in the posterior part of the lips and gums and in the hard palate.

We have found a primary pallor with very considerable variation in the strength of the current. Thus in one case primary flushing was first obtained with the index of the secondary coil at 9 cm. from the primary coil; with the secondary coil at 18 cm., a slight, though distinct, pallor was produced; moreover, the after-flush produced by the stronger stimulus was considerably shortened by applying to the nerve the weaker stimulus. In another case the secondary coil was gradually shifted in successive stimulations from 20 cm. to 6 cm. distance from the primary. In all the first effect was pallor; with the weaker stimuli this alone was obtained.* The shocks with the secondary coil at 6 cm. could scarcely be borne on the tongue; with the secondary coil at about 15 cm. they could not be felt on the tongue.

Although some of the results which we have just mentioned do not agree with those of Dastre and Morat, we wish to point out that they do not conflict with, but rather confirm, the main contention of these observers, viz., that the sympathetic contains both vaso-constrictor and vaso-dilator fibres for the bucco-labial region.

And from the unequal effects of a moderately strong stimulus on the different parts of the bucco-facial region, we may conclude that the proportion of constrictor and dilator fibres for the different parts

* Laffont ('Soc. de Biologie,' 1880, p. 341), on stimulating the uncut vago-sympathetic in an atropinised dog, found with all strengths of stimulation primary constriction followed by dilation, the primary constriction being briefer the stronger the stimulation. Apparently, however, the paralysis of the inhibitory fibres of the vagus by the atropin given was assumed, and actual observation on the point omitted; and Dastre ('Soc. de Biologie,' 1880, p. 348) attributes the previous pallor obtained on stimulating the sympathetic to a slowing or cessation of the heart-beat.

of the region is not the same; and from the unequal effects on different dogs we may probably conclude that the proportion of the two kinds of nerve fibres varies somewhat in different individuals, although it is possible that the results on which this conclusion is based may be caused by a temporary variation in the condition of the animal, for example, in the amount of anæsthetic given.

It was noticed by Bochefontaine and Vulpian and also by Dastre and Morat that in the cat and rabbit pallor, and not flushing, of the bucco-labial region is caused by stimulating the cervical sympathetic. Like these observers, we have not seen primary flushing with any strength of stimulus; the pallor is marked, except when, for any reason, the gums and lips are already pale; as a rule there is no marked after-flush, but the mucous membrane slowly regains its normal tint. On repeated stimulation of the sympathetic the bucco-labial region remains pale and shows very little alteration. In the course of our experiments upon the dog, we had occasionally seen a slight paling or flushing in the lips and gums on the side opposite to that on which the sympathetic was stimulated; in the cat and rabbit we have paid more attention to this effect, and we find that in these animals, stimulation of the sympathetic produces a bilateral effect. The pallor on the opposite side to that on which the nerve is stimulated is greater in the rabbit than in the cat, and is more obvious in the gum of the anterior part of the lower jaw than elsewhere. The degree of the pallor on the opposite side varies considerably in different individuals. Occasionally in the rabbit the pallor is complete on both sides, but in most cases it is much more marked on the side on which the nerve is stimulated. The bilateral action occurs with either sympathetic, although it may be more marked with one sympathetic than with the other; it occurs with all strengths of currents that produce any effect; it is best seen at the beginning of an experiment, for after repeated stimulation of the sympathetic the paling on the opposite side becomes less distinct, and it is much better seen in the anterior than in the posterior part of the lips and gums. In the rabbit a little care must be taken not to stretch the lips too much during the experiment, since this of itself may cause some pallor in the gums. We have also seen some bilateral pallor in the tongue, especially in the tip on stimulation of one sympathetic; but we have paid attention to this in a few experiments only.

Experiment I.

Rabbit (C. p. 34). Chloral. Chloroform and ether. Sec. coil at 9 gives shocks rather weak to tongue.

12.13. Tie and cut left sympathetic (separated from depressor) in middle of neck.

Stim. sy., $c = 9$, for 20 secs.; bilateral pallor in upper and lower lips; in

the anterior part of the lower lip the pallor is nearly equal on the two sides, in the upper lip the paling on the opposite side to that stimulated is distinct, though slight.

12.30. Cut right sympathetic and both vagi at the level of the upper part of the larynx.

Stim. sy., $c = 9$, for 20 secs. Bilateral pallor in upper and lower lip as before.

We may mention that, notwithstanding the difference in the dog on the one hand, and the cat and rabbit on the other, in the effect of stimulating the sympathetic on the bucco-facial region, a small dose of nicotin causes in each case a primary flushing in the region; in the dog the flushing is most intense, in the cat and rabbit it is comparatively slight, and may be very brief. Of this we shall have more to say in a later paper on the general action of nicotin.

Having thus given the effects which may be expected to follow stimulation of the sympathetic in the neck, we may now proceed to consider the order in which they cease on injecting into the blood-vessels small doses of nicotin. Our experiments have been made upon the rabbit, cat, and dog. As a rule, in any one experiment, a few only of the effects can be accurately observed. Thus, in order to observe with certainty a slight dilation of the pupil, it may be necessary to pull back the eyelids, in which case a slight movement of the eyelids, if such were caused by the stimulation, might escape observation. It has appeared to us that the effect of stimulating the sympathetic on the movements of the eyelids, the eye, and especially on the nictitating membrane, diminishes with the amount of the anæsthetic given. At any rate, in the rabbit and cat we have occasionally observed so little effect on the nictitating membrane to be caused by stimulating the sympathetic, that no certain conclusion could be drawn from the absence of such effect after giving nicotin.

It is necessary, then, to note carefully to what extent the various effects which may be produced by stimulating the sympathetic are in fact produced, immediately before the introduction of nicotin.

In nearly all cases the sympathetic in the neck was ligatured and cut. This was done, in the first place, to avoid reflex action, and, secondly, in the hope that, since section of the sympathetic commonly produces the opposite effects of stimulation, the effects of stimulation might thereby become more marked. It has often been noticed that section of the sympathetic produces a transient slight effect only, or even none; this was the case in most of our experiments, so that at the time of injecting nicotin, the ears and pupils on the two sides were alike, occasionally the ear being flushed but the pupil not contracted, or the pupil being a little contracted but the ear not flushed, on the cut side.

We have mentioned above that we have sometimes made observa-

tions on the progressive paralysis of the different sympathetic actions by giving a dose insufficient to paralyse them all, and sometimes by giving a larger dose and noting the progressive recovery. The former method is more troublesome, but brings out greater differences than the latter. The order of recovery is inversely as the order of primary paralysis.

The Rabbit.—As anæsthetics we have used chloral, and afterwards chloroform and ether, or, more rarely, chloral and morphia. In the anæsthetised animal the eyes are directed forward, and the pupils are rather large; after nicotin has been injected the eyes are directed forward, and the pupils are in nearly all cases smaller than previously. With regard to the relative time of paralysis of the secretory fibres in the cervical sympathetic, we have made no observations in the rabbit.

The easiest comparison to make is that between one of the changes which occur in the eye and the pallor of the ear. In the following experiment the comparison is made between the dilation of the pupil and the constriction of the central artery of the ear. The difference between the ease and duration of the paralysis of these two actions, though always appreciable, is nevertheless sometimes slight. The experiment we quote shows, perhaps, the maximum difference which we have observed.

Experiment II.

Rabbit (C. p. 7). Chloral given. Right sympathetic tied and cut in middle of the neck. Stimulation of the sympathetic with a weakish current ($c = 10$) produces dilation of the pupil and constriction of the arteries of the ear.

1.59. Inject 5 *mgm. nicotin.* into left jugular vein.
2.4. Stim. sy. for 20 secs., $c = 10$; no dilation of pupil, slight pallor of ear.
2.7. Stim. sy. for 20 secs., $c = 9$; no dilation of pupil, fair pallor of ear.
2.10. Stim. sy. for 30 secs., $c = 8$; no dilation of pupil, slight pallor of ear.
2.20. Stim. sy. for 20 secs., $c = 10$; no dilation of pupil, great pallor of ear.
2.27. Stim, sy. for 20 secs., $c = 10$; great dilation of pupil, and great pallor of ear.
2.35. Inject 5 *mgm. nicotin.*
2.40. Stim. sy. for 30 secs., $c = 10$; no dilation of pupil, slight pallor of ear.
3.12. Stim. sy. for 20 secs., $c = 10$; fair effect on both pupil and ear.
3.28. Stim. sy. for 10 secs., $c = 10$; nearly maximal dilation of pupil.
4.7. Inject 5 *mgm. nicotin.*
4.8. Stim. sy. for 10 secs., $c = 10$; great dilation of pupil and pallor of ear.
4.11. Inject 5 *mgm. nicotin.*
4.15. Stim. sy. for 10 secs., $c = 10$; great dilation of pupil and pallor of ear.
4.19. Inject 10 *mgm. nicotin.*
4.24. Stim. sy. for 30 secs., $c = 10$; no dilation of pupil, slight pallor of ear.
4.25. Stim. sy. for 60 secs., $c = 10$; no dilation of pupil, slight pallor of ear.
4.55. Stim. sy. for 60 secs., $c = 10$; no dilation of pupil, slight pallor of ear.
5.5. Stimulation of the sympathetic on the opposite side caused no dilation of the pupil, but a slight constriction of the vessels of the ear.

When we come to compare the effects more in detail, the difficulty is greater. This is especially the case in comparing the vaso-constrictor effects on the ear, mouth, and conjunctiva; for the pallor of all three, which often lasts for some time, and not for an equal time, as a secondary result of the nicotin makes it difficult to be certain of the beginning of vaso-constrictor action.

The movement of the nictitating membrane is more easily paralysed than the movement of the eyelids, and the latter is a little more easily paralysed than the dilation of the pupil. For the rest, the apparent order of ease of paralysis is vaso-constrictors of conjunctiva, vaso-constrictors of mouth, vaso-constrictors of ear; we say apparent order of paralysis, because we have instances from separate experiments, in which there has been, so far as could be judged, a simultaneous recovery in the dilation of the pupil and the pallor of the conjunctiva; pallor of conjunctiva and pallor of mouth; pallor of mouth and pallor of ear. The following experiments will illustrate the time differences observed :—

Experiment III.

Rabbit (C. p. 20). Chloral. Both cervical sympathetics ligatured and cut.

1.27. Inject into femoral vein 5 mgm. nicotin.

1.29. Stimulate left sympathetic; no effect.

1.31. Stimulate right sympathetic; no effect.

1.39. Stimulate left sympathetic; slight constriction of artery at base of ear, otherwise no effect.

1.40. Stimulate right sympathetic; slight constriction of artery at base of ear, otherwise no effect.

1.41. Stimulate left sympathetic for 60 secs.; fair constriction in artery of ear for about 45 secs.; no effect seen in lips.

1.46. Stimulate left sympathetic for 40 secs.; good constriction in artery of ear, gradual pallor of lower lip, chiefly on left side, but some on right. Slight after-flush.

1.50. Stimulate left sympathetic; slight dilation of pupil; conjunctiva already pale, shows no obvious change, but flushes a little when the stimulus has ceased. No movement of eyelid or nictitating membrane.

2.0. Stimulate right sympathetic. Marked pallor of conjunctiva, fair dilation of pupil; eye opens; no movement of nictitating membrane.

Experiment IV.

Rabbit (C. p. 27). Chloral. Right cervical sympathetic ligatured and cut. With secondary coil at 8 ($c = 8$) the shocks are distinctly felt on the tongue.

2.52. Inject 5 mgm. nicotin into crural vein.

3.20. Stim. sy., $c = 8$; usual effects, except that movement of eyelids very slight and movement of nictitating membrane only just perceptible.

3 42. Inject 1 c.c. 1 p. c. curari.

3.47. Stim. sy., good effects, except on nictitating membrane.

3.51. Inject 5 mgm. nicotin.

3.57. Stim. sy., 20 secs.; good constriction of artery of ear; slight pallor of mouth; no other effects observed.

3.59. Stim. sy., 30 secs.; good constriction of artery of ear; slight pallor of mouth and of conjunctiva, no effect on eyelid or nictitating membrane.

4.0. Stim. sy., 60 secs.; complete pallor of ear, slight pallor of conjunctiva, no dilation observed in pupil, but it is now a little larger than at 3.57.

4.5. Stim. sy., 10 secs.; fair pallor of conjunctiva, moderate dilation of pupil.

4.7. Stim. sy., 10 secs.; eyelids open slightly (previous to the stimulation the eyelids were pressed together).

4.13. Stim. sy., 30 secs.; pallor in lips and mouth is bilateral, but chiefly on the stimulated side.

4.19. Stim. sy., 30 secs., $c = 6$; eye opens and pupil dilates well, no movement of nictitating membrane.

The Cat.—In the cat, the secretory nerve cells of the superior cervical ganglion are paralysed before any others. After a small dose of nicotin (3 to 5 mgm.), stimulation of the cervical sympathetic causes, for a short time, no secretion of saliva, but still causes, or may cause, all the other effects normally seen as the result of the stimulation. The difference in the ease and duration of paralysis is in this case very striking. On the other hand, there is often very little difference in the ease of paralysis of the nerve cells of the superior cervical ganglion, which are connected with other classes of nerve fibres. There are some differences which are constant, but which vary very considerably in degree. In the following experiment, the difference between the time of paralysis of the vaso-motor effects on the ear and the dilator effect on the pupil is the maximum we have found.

Experiment V.

Cat (C. p. 24). Chloroform given, then morphia subcutaneously, and occasionally chloroform and ether. Cannula in the duct of the left sub-maxillary gland. Sympathetic in neck tied and cut on left side. Cut left chordo-lingual. The pupil is rather large; stimulation of the sympathetic with a weakish current ($c = 9$) causes the nictitating membrane to be drawn back, the eye to open, the pupil to dilate, the artery of the ear to constrict, and a secretion of saliva.

12.53. Inject into crural vein, 5 *mgm. nicotin.* The injection causes, amongst other effects, those described above as resulting from stimulation of the sympathetic.

1.0. Stim. sy., $c = 9$. Moderate opening of eye, dilation of pupil, and constriction artery of ear; no secretion.

1.5. Stim. sy., $c = 9$; effects as before.

1.13. Stim. sy., $c = 9$; secretion also.

1.18. Inject 5 *mgm. nicotin.*

1.28. Stim. sy., $c = 9$; no effect.

1.45. Stim. sy., $c = 9$; slight constriction artery of ear, no effect on pupil or on secretion.

1.50. Stim. sy., $c = 9$; constriction artery of ear, and slight dilation of pupil.

1.53. Stim. sy., $c = 9$; as before, and eye opens a little.

2.5. Stim. sy., $c = 9$; as before, but still no secretion.

The effect of the sympathetic upon the nictitating mem

paralysed less readily than the other effects of the sympathetic on the eye; in some experiments we have found a very considerable, in others a very slight, difference. Experiment V is an instance of the latter case.

Experiment VI.

Cat (C. p. 30). Chloroform. Right sympathetic ligatured and cut.

3.25. Inject into crural vein 4 *mgm. nicotin.*

3.38. Stim. sy., $c = 9$; all the usual effects produced.

3.44. Inject 4 *mgm. nicotin.*

3.51. Stim. sy.; all the usual effects produced.

3.55. Inject 4 *mgm. nicotin.*

3.59. Stim. sy., 10 secs., $c = 9$; nictitating membrane withdrawn a little, no other effect.

4.0½. Stim. sy., 10 secs., $c = 8$; nictitating membrane slowly drawn back, no other effect.

4.1¼. Stim. sy., 5 secs., $c = 8$; same effect, and pupil slightly dilated.

4.2. Stim. sy., 5 secs., $c = 8$; as before, and slight contraction of lower eyelid and mouth observed.

4.4. Stim. sy., 60 secs., $c = 8$; little, if any, immediate effect on tongue, but a slight after-flush.

4.6. Stim. sy., 60 secs., $c = 8$; mouth chiefly observed, a little paling of tongue and lips at first, changing to slight flushing at end of stimulation; pupil, as before, shows slight dilation only.

In this experiment, the dilation of the pupil was noticed before the opening of the eye, but, in some other cases, we have not been able to satisfy ourselves that this occurred. And it is possible that the position of the eyelids, whether nearly closed, or half-open, as they usually are after nicotin, influences the result. Similarly, we have not been able to assure ourselves at what time, in relation to the dilation of the pupil, a paling of the conjunctiva, and a paling of the mucous membrane of the mouth, occurs. We are inclined to place them in order of ease of paralysis, as follows: paling of mouth, paling of conjunctiva, opening of eyelids, dilation of pupil. We have not made a comparison between the ease of paralysis of the sympathetic effect upon the withdrawal of the nictitating membrane and the constriction of the vessels of the ear.

The Dog.—When nicotin, even in large amount, is injected into a vein in the dog, there is a rapid recovery of the effect of stimulating the cervical sympathetic* as regards the constriction of the small arteries of the salivary glands, the dilation of the pupil, and the secretion of saliva. We have generally observed a slight difference in the time of recovery of these three effects, in the order in which they are mentioned above, but there are special difficulties in the way of determining the exact time when stimulation of the nerve begins

* *Cf.* Langley, 'Journ. of Physiol.,' vol. 11, 1889, p. 123.

to be effective on the constriction of the vessels of the ear and on the secretion of saliva.

The other effects of stimulating the cervical sympathetic are, however, more easily suppressed by nicotin. The one most readily abolished is the flushing of the lips. With regard to the relative ease of paralysis of the movements of the eye, eyelids, nictitating membrane, and pallor of the mucous membrane of the mouth, we have made a few experiments only, so that we cannot speak of them with much confidence. The order, so far as our experiments go (*cf.* Exp. VI), is movement of the eyelids, movement of the nictitating membrane, pallor of the lips.

Experiment VII.

Dog (C. p. 11). Morphia. Chloroform and ether.

Left sympathetic separated from vagus for about an inch below superior cervical ganglion, ligatured, and cut. With secondary coil at 10 ($c = 10$), the shocks are distinctly felt on the tongue, but are not strong; with secondary coil at 6 ($c = 6$), the shocks are strong to the tongue. Stimulation of sympathetic with $c = 10$ causes flushing of lips and gums.

1.26. Inject into femoral vein 50 mgm. nicotin.

1.29. Stim. sy., $c = 10$, 20 secs.; no effect on eye or lips.

1.34. Stim. sy., $c = 6$, 30 secs.; no effect on eye or lips.

1.36. Stim. sy., $c = 8$, 60 secs.; lips slowly become pale on both sides, but this may be the after-effect of nicotin; no other change. The eye quickly shuts on touching the skin near it.

1.43. Stim. sy., $c = 8$, 60 secs.; pupil dilates—it was large before stimulation.

1.52. Stim. sy., $c = 8$, 60 secs.; pupil dilates readily, no other change observed; on left side lips are very pale, and the nictitating membrane is partially drawn back; on right side lips are pinkish, and nictitating membrane is $\frac{1}{3}$ to $\frac{1}{2}$ way over the eye.

2.10. Stim. sy., $c = 8$, 60 secs.; edges of lips become paler.

2.16. Stim. sy., $c = 8$, 60 secs.; momentary movement of nictitating membrane.

2.35. Stim. sy., $c = 12$, 30 secs.; eye opens.

2.37. Stim. sy., $c = 7$, 30 secs.; lips slightly flush for 10 to 15 secs., then become pale.

An interesting result is often obtained by stimulating the sympathetic after a rather larger dose of nicotin; in this case, the pupil is rather large, the eye is turned forwards, and the eye is open, but not widely; stimulation of the sympathetic then causes the eyelids slowly to approach one another, *i.e.*, the eye, instead of opening, becomes more closed. The movement is chiefly in the lower eyelid. On ceasing the stimulation the eye gradually opens to its previous extent. The closing of the eye on stimulating the sympathetic occurs at a time when the stimulation still produces dilation of the pupil and secretion of saliva. It will be remembered that Rogowicz* observed occasionally a similar closing of the eye in the dog when the sym-

* 'Archiv f. d. ges. Physiol.' (Pflüger), vol. 36, 1885, p. 7.

pathetic was stimulated several days after section of the facial nerve. He attributed it to a contraction of the orbicularis palpebrarum. It is possible that the sympathetic has nerve fibres stimulation of which causes closure of the eye, as well as fibres stimulation of which causes opening of the eye, and that the nerve cells in the superior cervical ganglion connected with the former are less easily paralysed by nicotin; but there is no decisive evidence of this, and the closure obtained may be explained in other ways.

Summary.

Generally speaking, stimulation of the cervical sympathetic in the dog with minimal effective shocks causes pallor in the lips and gums; with weak to moderately strong shocks, primary pallor followed by flushing; with strong shocks, as shown by Dastre and Morat, primary flushing, but the extent and duration of the primary effect and of the secondary effect, if there is any, varies in different dogs.

In the rabbit and cat, stimulation of the cervical sympathetic always causes, as shown by Bochefontaine and Vulpian, primary pallor in the lips and gums, and the after-flush is not great. The pallor we find is bilateral; the degree of the pallor on the opposite side to that stimulated varies in individual cases, and can be seen on the tongue, as well as on the lips and gums.

On injecting nicotin into a vein, certain of the normally occurring effects of stimulating the cervical sympathetic cease before the others, *i.e.*, since all the effects can still be produced by stimulating the fibres running from the superior cervical ganglion, the nerve cells in the ganglion, which are connected with different classes of nerve fibres, are paralysed with different degrees of ease by nicotin.

Arranging the various effects in the order of ease of paralysis, we have:—

Rabbit.

(1.) Withdrawal of the nictitating membrane.
(2.) Opening of eye.
(3.) Dilation of pupil.
(4.) Constriction of blood-vessels of conjunctiva.
(5.) Constriction of blood-vessels of lips and gums.
(6.) Constriction of blood-vessels of ear.

In one or two cases, no difference in the ease of paralysis between the bracketed actions has been observed.

Cat.

(1.) Secretion from sub-maxillary gland.

(1)
(2.) Opening of eye.
(3.) Dilation of pupil.
(4.) Constriction of blood-vessels of conjunctiva.
(5.) Constriction of blood-vessels of mouth.

(2) { (6.) Constriction of blood-vessels of ear.
(7.) Withdrawal of nictitating membrane.

(1) Constant differences between these have not been observed.

(2) These have not been directly compared, but in separate experiments each has been obtained when (1.) to (5.) were no longer seen.

Dog.

(1.) Dilation of arteries of bucco-facial region.
(2.) Movements of eye and opening of eyelids.
(3.) Withdrawal of nictitating membrane.

(1) { (4.) Constriction of arteries of gums and lips.
(5.) Dilation of pupil.
(6.) Secretion from sub-maxillary gland.
(7.) Constriction of blood-vessels of the sub-maxillary gland.

(1) Differences between these have not always been observed.

At a certain stage of nicotin poisoning, when stimulation of the sympathetic does not cause withdrawal of the nictitating membrane, but does cause dilation of the pupil, a partial *closing* of the eye is obtained by stimulating the sympathetic.

It will be noticed that in each animal nicotin abolishes most of the effects of stimulating the cervical sympathetic at very nearly the same time. With regard to these, we think that there is only a *primâ facie* case for regarding the differences observed as due to an unequal paralysis of the nerve cells of the superior cervical ganglion, for it is possible that the differences may be due to an unequal tonic stimulation reaching the parts by nerve fibres other than the sympathetic. But the greater differences observed, for instance, between the secretion of saliva and the dilation of the pupil in the cat, the flushing of the lips and the constriction of the vessels of the submaxillary gland in the dog, we do not think can be due to such a cause, and we attribute them to an unequal paralysing action of nicotin upon the nerve cells of the superior cervical ganglion.

The Society then adjourned over the Easter Recess to Thursday, April 17th.

Presents, March 27, 1890.

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CROONIAN LECTURE.—"On some Relations between Host and Parasite in certainEpidemic Diseases of Plants." By H. MARSHALL WARD, F.R.S., Professor of Botany, Royal Indian Engineering College, Cooper's Hill. Received and read February 27, 1890.

Introduction—Relations between Physiology and Pathology.

I thought I could not better respond to the honour of the invitation to give the Croonian Lecture this year than by choosing a subject from the domain of plant pathology, which should, at least, have the merit of being of general interest and importance, and it seemed probable that an account of some of the more conspicuous features and recent results of the study of certain fungoid diseases might be so placed before you that it should illustrate not only the kind of progress which plant pathology is making, but also show how dependent that progress is, and must be, on the advances of physiology. Moreover, I hope to be able to demonstrate that the connexion between these two modern branches of science is (in botany, at any rate, and I have no reason to doubt that the truth applies to the animal kingdom as well) so close and so mutual that the problems which arise daily appeal to students of both departments, and necessitate that each shall know what the other is about. This, of course, is not the same as saying that either branch of study is deficient in its special questions; but it cannot be too much insisted upon that, while the facts and generalisations of pathology often throw light on physiological questions, the enquirer into the pathology of plants has to pause at almost every step and ask some question in physiology, and his progress may be slower or more rapid in proportion as the answer is obscure or the reverse.

For, after all, the pathology of plants embraces those phenomena of abnormal life-processes which can go on in the long series of changes between normal, healthy, vigorous life, and the cessation of that life as such, *i.e.*, death; and it is obviously impossible to study these abnormal life-processes (pathological) without reference to the normal ones (physiological). In other words, then, pathology is the study of disturbed or abnormal physiological processes, and I thought that it would be possible to interest you in some of the phenomena of abnormal plant life, and especially in the working of some of these factors which result in producing certain diseases, in which fungi play the prominent part, and which occasionally assume the nature of epidemics so suddenly that the phenomena continually prove too much for the inherent credulity of those who are not in the habit of

investigating complex chains of causation, and give rise to speculations of the most superstitious description.

The Diseases of Plants and their Classification, &c.

The diseases of plants have been classified in various ways, at different times, and by different observers. Passing over the earlier attempts,* based for the most part on errors which were natural at

Fig. 1. A young coffee plant (reduced), the leaves of which are badly infected with the Uredinous fungus *Hemileia vastatrix*. The paler spots are bright orange-yellow, the centre gradually turns brown, and then black as the tissues are destroyed; the granular appearance on the younger spots is due to the spores of the fungus. Such yellowing of the leaves is a common symptom of such diseases.

* An excellent account of the earlier writers appeared in the 'Gardener's Chronicle,' 1854, from the pen of the late Rev. M. J. Berkeley, F.R.S.

the time, but some of which seem almost incomprehensible now, as some of our present errors will appear in the future, it may be said that no very exhaustive survey of these diseases as a whole was possible until comparatively recent times. The successive attempts of modern authors* have been almost entirely along one or other of two lines: they have classified the diseases either (1) according to the symptoms externally visible and the organs attacked, or (2) according to the causes which seem most concerned in producing the disease.

Whichever method is adopted, it is repeatedly found that large assumptions have to be made and recognised in order to bring given diseases into the sphere of treatment for the time being, and difficulties of very peculiar nature continually make themselves felt.

As an instance, we may take the well-known symptom of the appearance of yellow leaves. Not only are yellow leaves characteristic of many diseases due to fungi of the groups Uredineæ and Ascomycetes (*Peziza*, *Hysterium*, *Polystigma*, *&c.*), or to the attacks of insects (*e.g.*, Aphides), but they may indicate "something wrong" at the roots—want of drainage, over-drainage, or lack of some ingredient such as iron, or the presence of some noxious mineral, to say nothing of parasites (*Phylloxera*, *Melolontha*, *Agaricus melleus*, *&c.*).

In cold weather in the spring yellow leaves may mean that the temperature is too low for the production of the green chlorophyll; while frost is responsible for the yellowing of other leaves by a totally different procedure—the acid substances in the cells are enabled to diffuse through to the chlorophyll corpuscles and kill them.

Yellow leaves often indicate the access of too little sunlight, but they may be produced by too intense insolation and consequent destructive changes in the cells.

Leaves injured by acid gases and poisonous substances in smoke also turn yellow, and the yellow hue of autumnal leaves is well known, while we have numerous yellow varieties of leaves among cultivated plants, the causes for which are less clear.

These are by no means all the cases observable, but they will suffice to show how little can be inferred from a symptom which may be due to so many causes, operating alone or in combination, be it said. In fact, as a symptom, the yellowing of leaves is of scarcely any classificatory value, and we are driven to the conclusion that the leaves of plants react to most injuries by turning yellow.

It is much the same with other classes of disease named after the prominent symptoms. What is usually termed "canker," for

* The literature is for the most part in Hallier's 'Phytopathologie' (1868), Frank's 'Krankheiten der Pflanzen' (1880), and Sorauer's 'Pflanzenkrankheiten' (2nd ed., 1886).

FIG. 2. Oaks in the neighbourhood of a manufacturing town, the leaves of which were damaged by acid gases. The injury results in the production of yellow spots on the leaves, and the latter eventually turn wholly yellow and brown, and die. From a photograph taken August 8th, 1882.

instance, is in different cases referred by different authorities to the agency of excessively low temperature (frost*), or of insects,† or of fungi,‡ or of two of these combined, to say nothing of other causes. If we now ask how the matter stands with regard to the method of classifying diseases of plants according to their chief causes, the answers are no clearer. It is the custom to proceed somewhat as follows:—

There are, *first*, diseases due to the action of the non-living en-environment (soil, climate, mechanical injuries, &c.); and, *secondly*, diseases due to the attacks of living beings (parasitic insects, fungi, &c.).

Now, leaving out of account altogether certain totally unexplained diseases, such as some forms of "gumming," &c., it becomes apparent that we are liable to all kinds of errors unless we recognise that no one factor ever accounts for a disease; it is not so obvious, however, that the changes which result in disease are usually due to several factors acting in concert or successively, and I shall try to show that, even in marked cases, it is by no means always easy to decide which

* Sorauer 'Pflanzenkrankheiten' vol. 1, 1886, pp. 305—448.

† Frank, 'Krankheiten d. Pflanzen,' 1880, p. 719.

‡ Hartig, 'Lehrb. d. Baumkrankheiten,' pp. 89 and 109.

FIG. 3. The same oaks as those of fig. 2, photographed from the same spot on July 20th, 1888. The cumulative injury to the leaves in successive years results in the death of the trees.

of the co-operating factors are to be brought into the foreground, though, until this is decided, it may be a hopeless task to consider prophylactic measures. As examples of the complex interactions that may be met with in the first group of diseases as arranged above, we might consider the following :—

A soil is said to be unsuitable as regards aspect, or elevation, or steepness, but it will be evident that the degree of unsuitableness may vary with the depth and structure of the soil, and with the latitude, the proximity of mountain ranges or the sea, and other factors which influence the climate ; instances of disease, in the broad sense of the word, are frequent enough where two neighbouring crops or growths of the same species of plant suffer in very different degrees, owing to slightly different combinations of such factors of the environment; and the difficulty of referring the disease more especially to any one cause only increases with experience. Or, take the structure, &c., of the soil. It may vary in chemical composition, in capacity for retaining water, in physical texture, and so forth ; and the enormous differences to be met with are best known to those who have to cultivate large estates or continuously observe large tracts of country. But it is matter of general experience that the chemical composition of a soil is one of its least important features

within wide limits; much more important is the amount of water and air in it, and the way they are held there. These, especially under certain crops, affect the climate of the immediate locality, and all kinds of complexities result. To mention one only, there are certain combinations of soil and climate, &c., which result in the trees being "frost-bitten" whenever there are late spring frosts. In some cases it is found that mere drainage puts an end to the evil; this means not only a removal of water, but an increase of air in the soil and general elevation of temperature. In others it is noticed that the more shaded trees suffer most; this is in part because their tissues are more watery, and their cell-walls more delicate. In others the injury occurs on a particular side of the tree, and is ruled chiefly by the prevailing winds. Now, here is a problem of considerable complexity. Frost (*i.e.*, too low a temperature) is the agent directly concerned, but it accomplishes the injury because the shoots are too succulent, and the tissues too feebly developed, to resist a temperature which they would be perfectly able to resist if more carbohydrates had been formed under a brighter light, and if less water containing more oxygen had ascended their stems, and so forth.

It is at least difficult to class such cases, and they arise every day. Who would have suspected that one result of bringing the larch down from its mountain home would be to render it more liable to injury from certain pests kept in abeyance on its native Alps, because it is stimulated to put forth its young leaves when the insects are about, which puncture the cortex and afford means of entrance for certain parasitic fungi?

On turning attention to the diseases referred to the action of living organisms, we meet with difficulties rather greater than less, and it is chiefly on account of these that so many wild hypotheses are current as to this class of diseases.

Omitting more than a mere reference to the diseased or weakened conditions due to competition with weeds, and with overbearing associates, such as *Thelephora laciniata*, which may overshadow young Conifers, and eventually kill them simply by depriving them of light,* and to the various parasitic Phanerogams such as *Loranthus*, the mistleto, dodder, &c.,† we come to an enormous series of diseases due to parasitic insects (and other animals) and fungi.

The chief difficulty connected with the investigation of diseases induced by fungi is due to the double set of complications involved. It is difficult enough to unravel the tangled skeins of causes and effects

* R. Hartig in 'Untersuchungen aus d. Forstbot. Institut zu München,' 1880, p. 164.

† See Solms-Laubach, "Ueber den Bau und die Entwicklung der Ernährungsorgane paras. Phaner." ('Pringsheim's Jahrbücher,' vol. 6, 1867–8, p. 509).

in the case of the comparatively simple diseases referred to above; but when the problem consists in disclosing the life-history of a microscopic fungus on the one hand, and then in discovering its relations to the plant (the biology of which is always assumed to be known) on or in which it passes the whole or part of this life-history, on the other, the matter rapidly attains unexpected proportions. Yet this is never the whole, or necessarily the major part, of the *real* problem—the nature of the disease—and before that can be even approximately solved we have to obtain an insight into the influence of the non-living environment on both the host-plant and the parasitic fungus, an inquiry which may assume appalling proportions before it is far advanced.

Nor is this the end, though it is quite sufficient to account for the fact that we never know all about any of these diseases.

There is a factor—or set of factors—which always tends to baffle the inquirer into these matters, and that is the internal disposition* of the parts of the organisms concerned; call it what we will—constitution, inherited disposition, &c.—the fact remains that the host-plant and the parasite alike exhibit peculiarities of behaviour that cannot be explained in the present condition of science as directly due to the action of any external agency of the environment, although we are no doubt right in concluding that it is the outcome of the cumulative results of the vicissitudes of the species and its ancestors in the long past.

But it is just the reactions of this constitution, and its variations induced by changes in the physical environment, which are so often and so persistently overlooked, although the attempt to understand any disease is hopeless, unless we take them into consideration. I hope to show, in the course of this lecture, how the modern study of the pathology of plants differs in methods from that of our predecessors, especially in this very particular—the recognition of the reactions of the host to its living and non-living environment, as apposed to the reactions of the parasite to *its* living and non-living environment, and, further, of the truth that disease is the outcome of a want of balance in the struggle for existence just as truly as normal life is the result of a different poising of the factors of existence.

Of course, inasmuch as the abnormal state of affairs, while detrimental to the host, is the best possible for the parasite, we have here the elements of a paradox; but there is no real confusion of ideas here; we are concerned with a particular case, illustrative of the struggle for existence, in which a given set of variable factors of the environment favour one organism at a time when they disfavour another.

* Sachs, 'Lectures on the Physiology of Plants,' pp. 189—204.

The Host-plant, and the Behaviour of its Normal Tissues.

I begin by briefly calling attention to the healthy tissues of a normal green flowering plant, and we need only consider for the moment what is going on in the parenchyma cells of a leaf or stem, such as every one knows the anatomy of. In a selected piece of such tissue we find the mass cut up into a number of thin-walled chambers, the cells, each of which contains a lining of living, colourless protoplasm, with strands or plates of the same running across; in this protoplasm are embedded the nucleus and the green chlorophyll

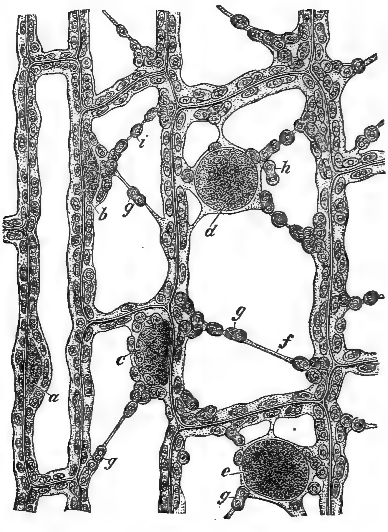


FIG. 4. Portion of the cell-tissue of a higher plant, in longitudinal section and highly magnified. Each of the cells is bounded by the cellulose cell-wall; and this is lined by the protoplasm in which are embedded the nucleus (*a*—*e*) and the green chlorophyll corpuscles (*g*—*i*). This protoplasm encloses the cell-sap, and strands of the former may pass across (as at *f*), or plates of protoplasm may separate the sap of one part of the cell from that of another (Kny).

corpuscles. In the large vacuoles or sap-cavity of each cell is a clear liquid, the cell-sap, consisting of water with small quantities of mineral salts, dissolved gases, organic acids, and salts and other crystalline and non-crystalline substances in various proportions at different times.* Of course, I need not here enter into a long de-

* On the subject of the extreme complexity of the cell-sap and protoplasm, see Pfeffer, "Beiträge zur Kenntniss der Oxydationsvorgänge in lebenden Zellen" 'Abhandl. Math.-phys. Classe Sächsischen Gesellsch. d. Wiss.,' vol. 15, 1889, pp. 455—466).

scription of the histological peculiarities of the cell, and it will probably suffice to remind you that great differences occur in detail as to the size of the cell, the thickness of the wall, number and sizes of the chlorophyll corpuscles, and the presence or absence of colouring matters, crystals, various organised bodies, and so forth. Finally, it will be remembered that all the parts—cell wall, nucleus, chlorophyll corpuscles, and protoplasm generally*—are more or less thoroughly saturated with water, and that aqueous vapour and gases will be found in varying proportions in the passages between the cells, and continuous with the atmosphere, on the one hand, and with the water in the roots and soil, on the other.

Let us now inquire what these normal living cells are doing when they still form an integral part of the tissues of the healthy plant.

In the first place, they are respiring. That is to say, the protoplasm absorbs oxygen gas† brought to it in the water from the roots, from the intercellular spaces which communicate with the atmosphere by means of stomata and lenticels, and from the chlorophyll corpuscles when they are assimilating in bright light. This oxygen enters in solution into the protoplasm, and combines with some of the bodies which for the time being enter into the composition of this complicated structure. The effects of these unions of the oxygen are expressed in molecular disturbances in the protoplasm: some bodies are broken down, others enter into new unions. Finally, the disturbing actions of the energetic oxygen result in the combustion of certain carbon-compounds to carbon dioxide and water, and these escape from the field of action: such combustion implies the liberation of energy, and we recognise this in the complicated movements and life-processes set up in the protoplasm and in the rise of temperature, which can be proved to take place.‡ One point of importance should be insisted on from the first. When the oxygen-molecule enters the protoplasm, it must be pictured as coming into a busy arena, where numerous but definite possibilities are presented to it, and although we are not in a position to trace its movements,§ and the intermediate effects of these, in detail, the evidence shows that while the quantities produced accord with the general view that it is such substances as glucose,

* For particulars as to these, *cf.*, *e.g.*, Zimmermann, "Die Morph. und Physiol. der Pflanzenzelle;" Schenk's 'Handbuch,' vol. 3, Heft 2, pp. 497—700; and Noll, "Die wichtigsten Ergebnisse der botanischen Zellenforschung in den letzten 15 Jahren" ('Flora,' 1889, pp. 155—168).

† *Cf.* Sachs, 'Lectures on the Physiology of Plants,' pp. 395—408; Vines, 'Physiology of Plants,' pp. 195—202; Pfeffer, 'Pflanzenphysiologie,' vol. 1, pp. 346—363.

‡ Rodewald, 'Pringsheim, Jahrb. f. wiss. Bot.,' vol. 17, 1886, p. 338; vol. 19, 1888, p. 221.

§ It may be regarded as certain that for respiration it does not suffice for a body to be merely in the protoplasm (see Pfeffer, 'Oxydationsvorgänge,' pp. 489—490).

and similar carbohydrates, which yield the fuel and energy—as they do in ordinary combustion—nevertheless, we must not fall into the error of supposing that so much sugar or starch in the protoplasm is forthwith and simply oxidised to carbon dioxide and water, nor may we conclude that the process is one of simple and direct oxidation at all.*

In the first instance, it is chiefly owing to the vagaries of the oxygen-molecules in the living protoplasm, that the latter exercises the processes of *metabolism*, the second group of functions we have to consider.

The metabolic processes which can be referred to the changes brought about during respiration† result in two series of events. On the one hand, compounds of various kinds pass out of the protoplasm—the arena of metabolic activity—into other parts of the cell, and especially into the cell-sap; and, on the other hand, bodies of comparatively simple constitution are brought from the cell-sap and elsewhere into the arena of activity, and there worked up into more complex bodies. It is impossible to separate these two sets of processes; but, if we abstract them mentally, for purposes of simplicity, we may say that the following series of events important for our present purposes are taking place.

Carbohydrates, especially in the form of glucoses, are being taken up into the protoplasm, and built up into the structure of its substance: here, owing to the attacks of the oxygen of respiration, the structures into which they enter are more or less broken down—as before said, not necessarily merely oxidised as such or directly—and the complex into which they have temporarily entered becomes decomposed, again to be built up anew by the aid of more carbohydrates, and so on repeatedly.

Among the temporary products of these *destructive* processes, in the complex alternations of building up and breaking down here going on, we find certain nitrogenous compounds (amides and allied bodies) like asparagin, leucin, glutamin, &c., playing important parts. The evidence goes to show that so long as plenty of carbohydrates are at the disposal of the protoplasm, these amide-bodies are again worked

* For the older literature, see Pfeffer, 'Pflanzenphysiologie,' vol. 1, p. 353; Sachs, 'Lectures on the Physiology of Plants,' pp. 395—408: and Vines, *op. cit.*, p. 214. Then consult Palladin, in 'Berichte d. Deutsch. Botan. Gesellsch.,' 1886, p. 322; 1887, p. 325; 'Botan. Centralblatt,' vol. 33, 1888, p. 102; Pfeffer, "Beiträge zur Kenntniss der Oxydationsvorgänge in lebenden Zellen" ('Abhandlg. Math.-phys. Classe Sächsischen Gesellsch. d. Wiss.,' vol. 15, No. 5, 1889, pp. 375—518, especially 480—500), where the more important special literature is quoted.

† Strictly speaking, metabolism includes all the chemical changes in the protoplasm which constitute it living substance; it is a mere convention to speak of different kinds of metabolism, and to separate carbon-assimilation as a special function.

up with them into the more complex bodies, to be again broken down, and repeat the process,* and so on.

If, however, for any reason a lack of these carbohydrates occurs, then these amide-bodies increase for the time being, and the protoplasm suffers accordingly; in fact, it undergoes further decompositions as a result of starvation.

The evidence also goes to show that organic acids (such as malic, citric, tartaric, oxalic, &c.) are formed in the protoplasm, and accumulate in the cell-sap during these metabolic processes, as products of incomplete oxidation, and their variations in quantity depend greatly on the activity of these metabolic processes, and, therefore, on the intensity of respiration.† A fact of primary importance for us is that these organic acids increase considerably in amount under conditions which lead to less complete oxidation, and, conversely, they decrease when certain oxidation-processes in the cell are promoted. In other words, they are continually being formed and destroyed in metabolic changes, and sometimes one process, sometimes another, predominates.

As a third group of life-processes which our selected cells would exhibit, we may regard the phenomena of *growth;* processes which are intimately dependent upon *respiration* and *metabolism*, and, indeed, inseparable from them in life.

For our present purpose, it suffices to regard growth‡ as consisting in an extension of the still soft cellulose cell-walls, which tends to increase the area of the membrane at the expense of their thickness, and in a compensating increment in their thickness due to the activity of the protoplasmic lining in secreting and laying on cellulose on the inside of, or even in the structure of, the wall. Passing over the fact that the secretion of this cellulose is another manifestation of metabolic activity on the part of the protoplasm, it is important to notice that growth is only possible so long as respiration is proceeding, and so long as the cell is turgid.§ Now turgidity depends on the

* See E. Schulze, 'Landwirthschaftliche Jahrbücher,' 1876, vol. 5, p. 848; Borodin, 'Bot. Zeitg.,' 1878, col. 801; Palladin, "Ueber Eiweisszersetzung in d. Pflanzen," &c.' ('Ber. d. Deutsch. Bot. Gesellsch.,' 1888, p. 205, see p. 212). Further literature in Pfeffer, 'Pflanzenphys.,' p. 301.

† See especially Warburg, "Ueber die Bedeutung der organischen Säuren für den Lebens-process der Pflanzen" ('Unters. aus d. Bot. Inst. zu Tübingen,' 1886—88, vol. 2, pp. 53—152, where the literature is collected up to date; and Palladin, "Athmung und Wachsthum" ('Ber. d. Deutsch. Bot. Gesellsch.,' 1886, pp. 322—328), and the same on "Bildung der organischen Säuren in den wachsenden Pflanzentheilen" (*ibid.*, 1887, pp. 325—326).

‡ For a general account of growth, *cf.* Sachs, 'Lectures,' pp. 411—424 and 567—569.

§ See de Vries, 'Unters. über die mechanischen Ursachen der Zellstreckung,' Leipzig, 1877, and the literature there quoted.

presence in the cell of water under pressure: that is to say, in a turgid cell there is in the sap cavity sufficient water not only to supply all the demands of the cell-walls and protoplasm, but to keep them distended as well, and this to such a degree that the cellulose walls, with their lining of protoplasm, are positively stretched in opposition to the elastic resistance offered by the former. Recent researches have proved that this excess of water is largely due to the osmotic attraction exerted by the organic acids and their salts dissolved in the sap of the cell,* and since we have seen that the formation and destruction of these acids depend on the processes connected with oxidation and respiration, we obtain a futher glimpse into the complicated correlations here concerned. For our purpose the important points are that, during active turgescence, the growing cells tend to become very watery and their cell-walls to be thinned by stretching, and this in spite of the activity of the protoplasm in adding new materials; while bodies such as soluble amides and organic acids are being formed continuously and in relative and varying abundance, to undergo further changes in the never ending turmoil of metabolism, as already indicated.

But it is evident that these processes of respiration, destructive metabolism, and growth must sooner or later come to an end if the stores of carbohydrates fail, since these are the substances which ultimately supply the fuel for respiration, and which form the raw materials by means of which new protoplasm may be constructed; and it is well known that the plant respires and grows to death if placed in such circumstances that no new supplies of these substances are possible. We must remember that we are concerned with normal green cells, however, and we have now to consider the new set of events due to the assimilative action of the chlorophyll corpuscles to which these cells owe their colour. It is not necessary to remind you that this process of carbon *assimilation*† consists in the coming together of carbon dioxide and water in the green corpuscles, where, by means of energy obtained in certain rays of sunlight, the molecules of the carbon dioxide and water are torn asunder and eventually in part rearranged; speaking generally, we may say that some of the constituents (oxygen‡) escape, while others (carbon, hydrogen, and

* De Vries, "Ueber die Bedeutung der Pflanzensäuren für den Turgor der Zellen" ('Bot. Zeitg.,' 1879); also Palladin, "Bildung der organischen Säuren in den wachsenden Pflanzentheilen" ('Ber. d. Deutschen Bot. Gesellsch.,' 1887, p. 325). Other literature will be noticed where necessary as we proceed.

† For a general account of carbon assimilation, see Sachs, 'Lectures on the Physiology of Plants,' especially pp. 296—323.

‡ This oxygen is not active (see Pfeffer, 'Beitr. z. Kenntn. Oxydationsvorgänge,' p. 478).

oxygen) form new combinations, which result in the production of carbohydrates, which then separate from the protoplasm.*

We are here, of course less concerned with the difficulties which beset the questions, what rays of light are concerned in this process, how their energy is employed in the chlorophyll, and what part the chlorophyll itself takes directly in the process; or with questions as to the exact products formed during the putting together of the carbohydrate in the protoplasm of the chlorophyll corpuscle, and so on, than with certain well-established facts and conclusions, such as the following.

The process of building up the products obtained by the decomposition of the carbon dioxide and water in the protoplasm into carbohydrates goes on continuously in the sunlight, so long as it is sufficiently intense, and the excess beyond what is immediately required for the nourishment and respiration (*i.e.*, the maintenance of metabolic activity) of the living substances of the cell takes the final form (usually†) of starch. Free oxygen escapes all the time, and, in so far as this is not absorbed for purposes of oxidation, there and then in the cell, this oxygen goes to enrich the atmosphere. Moreover, these temporary stores of starch are continuously being transformed into soluble glucoses, by means of diastatic ferments‡ in the protoplasm; this process goes on day and night, and its result may be easily demonstrated in the case of leaves removed from the plant after exposure to the sunlight during the day. After a few hours in a warm, dark, normal atmosphere, relatively large quantities of glucose are found in the cells, while the starch is disappearing. This glucose, I need hardly remind you, is the soluble movable form of the carbohydrates,§ and it is worked up again, so far as it is in excess of the

* The literature of this part of the subject is enormous, and dates from Priestley ('Phil. Trans.,' 1772) to the present time. It may be said to fall under four heads: (1) the nature and functions of chlorophyll; (2) the absorption of carbon dioxide and the evolution of oxygen; (3) the intensity and kind of light necessary; (4) the chemical processes which intervene between the coming together of the carbon dioxide and water and the production of the final visible product—starch. I shall, naturally, here refer only to such special literature as bears on the main subject of the present lecture.

† Sachs, 'Flora,' 1862, Nos. 11 and 21, and 1863, p. 33; also 'Bot. Zeitg.,' 1862, col. 366; Godlewski, 'Flora,' 1873, p. 378, and 'Arb. des Bot. Inst. in Würzburg,' 1873, vol. 1, p. 343. Again, Sachs, 'Arb. des Bot. Inst. Würzburg,' vol. 3, Heft 1, 1884; G. Kraus, 'Jahrb. für wiss. Bot.,' vol. 7, 1870, p. 511; Famintzin, 'Jahrb. für wiss. Bot.,' vol. 6, p. 34.

‡ Baranetzky, 'Die Stärkeumbildenden Fermente,' 1878.

§ Numerous interesting results have been obtained of late years confirming and strengthening our theory of carbohydrate assimilation: see Böhm ('Bot. Zeitg.,' 1883, col. 33), A. Meyer ('Bot. Zeitg.,' 1886, col. 81), Laurent ('Bot. Zeitg.,' 1886, col. 151), who proved that leaves deprived of starch can form it from various sugars, glycerine, &c.; also Wehmer ('Bot. Zeitg.,' 1887, col. 713), O. Löw ('Ber. d. Deutsch. Chem.

immediate requirements of the living protoplasm, into the form of reserve starch, &c., by the protoplasm.*

At certain periods, therefore, the cells may contain relatively large quantities of this soluble, nutritious, and easily oxidised glucose.

We have still to refer shortly to another set of events taking place in the normal living cells, the connexion of which with the above simultaneous functions will be obvious. This is the *passage of water* from one cell to another, a process depending essentially upon the modified evaporation—*transpiration*†—going on at those surfaces of the cell walls which are in contact with the air in the intercellular spaces, &c., and the rapidity and magnitude of whose movements depend on a variety of circumstance.

This water comes from the vascular system, by which it is brought up from the soil after being absorbed by the root-hairs, and it contains traces of the necessary mineral salts—chiefly sulphates, nitrates, and phosphates of calcium, magnesium and potassium, in small, and varying quantities—as well as dissolved gases. Whether the oxygen dissolved in the water absorbed at the root reaches the cells higher up in the plant or no, it is at least clear that the water in these cells becomes oxygenated by contact with the atmospheric air which penetrates into the intercellular spaces, *viâ* the stomata and lenticels.‡ Moreover, it is impossible to doubt that oxygen reaches the water in the cells from the assimilating chlorophyll corpuscles. However, we are not confined to inferences in this connexion, since Pfeffer has conclusively shown that free oxygen does exist in the cell-sap§ in the normal condition.

The importance of this matter for my purpose is that the move-

Gesell.,' 1886, p. 141), Bokorny ('Ber. d. Deutsch. Bot. Gesellsch.,' 1888, p. 116), who confirmed the above and proved the same for methylal, methyl alcohol, glycol, &c.; and Saposchnikoff ('Ber. d. Deutsch. Bot. Gesell.,' 1889, p. 258). The organic acids cannot be employed with the same results (see Wehmer, *op. cit.*, p. 713), though they can be absorbed and oxidised in the living cells (Warburg, *op. cit.*, pp. 112—113) more rapidly than they are decomposed outside the plant.

* See Schimper, "Unters. über die Entstehung der Stärkekörner" ('Bot. Zeitg.,' 1881, p. 881); A. Meyer ('Bot. Zeitg.,' 1880, Nos. 51 and 52).

† For the general exposition, see Sachs, 'Lectures on Physiology of Plants,' pp. 246—254, and the text-books quoted. Then Kohl, 'Die Transpiration der Pflanzen,' &c., Brunswick, 1886; Eberdt, 'Die Transpiration d. Pflanzen und ihre Abhängigkeit von äusseren Bedingungen,' Marburg, 1889. The literature is collected by Burgerstein in 'Verhandl. d. K.K. Zool.-Bot. Gesell. zu Wien,' vol. 37, 1887; vol. 39, 1889.

‡ See Godlewski's explanation of the fine air-passages which run between the medullary ray-cells and place them in communication with lenticels (Pringsheim's 'Jahrb. f. wiss. Bot.,' 1884, pp. 569—630).

§ 'Unters. a. d. Bot. Inst. in Tübingen,' vol. 1, p. 684, and "Beitr. zur Kenntniss der Oxydationsvorgänge in lebenden Zellen" ('Abhandl. der Kgl. Sächsischen Gesell. d. Wiss.,' vol. 15, 1889, p. 449).

ments of water, chiefly due to transpiration, but also incidentally caused by local decomposition, osmotic absorptions, &c., are effective in bringing about aëration of the tissues; of course this aëration (or ventilation) is not to be confounded with the movements of free gases, due to diffusion or to expansions or contractions due to changes of temperature.*

These, then, are some of the changes which are continually and continuously going on in the living cells of the normal plant. Of course I have not attempted any exhaustive list, or even a complete sketch of the structures and processes met with in living cells, the purpose being simply to bring prominently into view certain features of importance to the matter in hand.

The Death of the Cell.

The next point to consider is, what changes are observed when such cells as the above are killed.† It appears to be of little moment

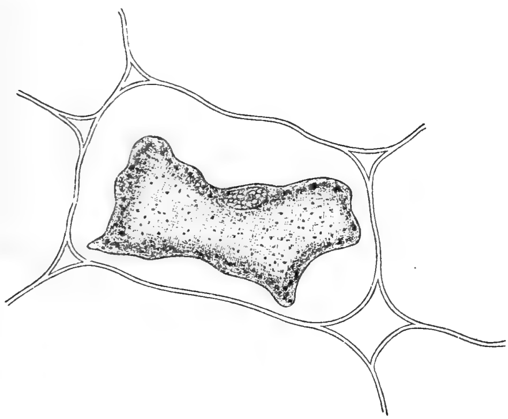


FIG. 5. A thin-walled parenchymatous cell killed by a few seconds' immersion in water at 75° C. The protoplast contracts from the cell-wall, carrying with it the nucleus and chlorophyll-corpuscles, and allowing the cell-sap to escape; the thin cellulose wall consequently becomes lax, and suffused with cell-sap. A similar result is brought about by longer immersion in water at lower temperatures (above 50° C.), or by very low temperatures, the action of poisons, &c. (Highly magnified.)

* For further discussion, see Pfeffer, 'Pflanzenphysiologie,' vol. 1, pp. 112—113, and the literature on transpiration.

† On this subject *cf.* Frank, 'Krankheiten der Pflanzen,' 1880, pp. 12—15; Detmer, in 'Bot. Zeitung,' 1886, No. 30; Pringsheim, in 'Jahrb. f. wiss. Bot.,' vol. 12, 1880 (pp. 47—50 of the separate copy); also de Vries, 'Untersuch. mechan. Ursachen d. Zellstreckung,' pp. 17—21.

how the killing is brought about, so far as the final appearances are concerned. We may place the cell, for instance, for a few minutes in hot water (say 55—80° C.), or expose it to very low temperatures, or to the vapour of chloroform, to acid gases, &c., and in each case the morphological changes are substantially the same.* In the first place, the movements of the protoplasm cease, and the granulation increases, while the whole contracts away from the cell walls into a more or less shrivelled, irregular lump. The cell-sap, previously held in the sap-vacuoles† under pressure in the turgescent living cell, now escapes, and suffuses the whole tissue, evidently because, the structure of the protoplasm being destroyed, it can no longer be kept in bounds as it was before. It would carry us too far to enter into the discussion as to what kind of changes the protoplasmic lining has undergone in its different layers; it suffices to note the fact that, whereas the living protoplasm was able to regulate the entrance of substances into the cell-sap and their escape from it, it is no longer able to do so when the cell has been killed, and the uncontrolled sap escapes as said. This sap is acid, often strongly so, and contains, among other things, certain bodies, known generally as chromogenes, which, on exposure to the air, undergo oxidation changes which result in the formation of brown colouring matters. We know very little about these chromogenes beyond the fact of their existence, but the evidence goes to show that they are unstable bodies of various kinds which are present in the cell-sap under such conditions that they are not directly oxidised by the passive oxygen dissolved in the sap; on exposure to the air, however, some substance in the sap acts as an oxygen-carrier, and they undergo the change of colour referred to.‡ The consequence of this is that the disorganised protoplasm, cell walls, &c., of the tissues thus suffused turn brown, resulting in the well-known colours of dead vegetable tissues. These changes are accelerated by the organic acids, which cause the chlorophyll grains to turn yellow, and then suffer further changes from the oxygen of the air.

It is, of course, unnecessary to remark that all the rhythmical series of processes connected with the living cell are now put an end to: respiration, metabolism, growth, assimilation have obviously ceased, as have all the other functions of the cell. Moreover, the evaporation of water is no longer controlled by the conditions imposed on it by

* This, of course, without prejudice as to the sequence of molecular changes which bring about the final result.

† See Pfeffer, 'Osmotische Unters.'; de Vries, "Studien üb. d. Wand d. Vacuolen," &c., in addition to the foregoing.

‡ See especially Pfeffer, "Beitr. zur Kenntniss d. Oxydationsvorgänge in lebenden Zellen," pp. 447—454, where the older literature is collected. The remarkable behaviour of these substances in the cell-sap suggests how extremely complex every part (even the presumably simplest) of the organism of the cell must be.

the protoplasm of membranes of living cells,* and if the weather becomes dry the dead tissues rapidly desiccate and shrivel.

In attaining the above described extreme, commonly called death, the normal living cell, in the condition commonly called health, passes through a series of vicissitudes which affect every part of it; but it is necessary to admit that the state called death and that called life, in the above discussion, are by no means definite and utterly distinct from one another—on the contrary, the very essence of life consists in its mobility, and the living cell is continually approaching and receding from the state termed death. In a certain sense, no doubt, death may be regarded as the cessation of life, but this does not help us, because the *crux* resides in determining when life ceases in the protoplasm. Of course we can lay our hands, as it were, on given cells or tissues of cells, and say these are "living," whereas others are "dead," but the difficulty is to decide when the one state passes into the other.

Between normal life, *i.e.*, the condition of affairs where the life-processes are going on actively, and the state of permanent death, then, there are all possible gradations: many of these gradations coincide with the phenomena of disease—pathological conditions—and it is towards this difficult domain that I have now to carry the discussion.

Variations in the Environment as affecting the Physiological Processes in the Host.

In describing the phenomena going on in what was termed the normal, living cell, I only hinted at the fact that variations, more or less periodic in nature, occur in the intensity of the processes, a truth which at once shows the difficulty of deciding what a normally living cell really is. But it is of the utmost importance to recognise that all the life-processes, and the changes dependent on them, are in their very nature variable. One set of factors which bring about the variations are internal and inherited, and very little is known of them beyond the fact of their existence, which is usually formally expressed by the admission that different plants differ in "constitution;" fortunately, this series of factors does not concern us at present, and it does not vitiate our general conclusions to assume that on the whole the differences in constitution between plants of the same species are so minute that they may be neglected.

The second set of factors is of much greater importance, because they give rise to pronounced and easily recognisable changes in the

* See Sachs, 'Physiologie Végétale,' pp. 253, for proof that water evaporates less rapidly from living cell-surfaces than from dead ones, and for literature (my edition is the French one of 1868).

plant. These factors are such as the following: changes of temperature, variations in the intensity of the light, differences in the amount of aqueous vapour in the atmosphere, &c., in short, the variable factors of the physical environment of the plant. That these affect the physiological processes in the cells is well known;* but what I have to do is to trace some of the effects, and show how they bring the living tissues into such conditions that they more or less readily resist or succumb to the attacks of certain parasitic fungi.

Taking the more or less arbitrarily chosen but convenient headings already employed—respiration, metabolism, growth, carbon assimilation, &c.—let us now see what kinds of effects the external agents referred to may produce.

Respiration, though it proceeds at very low temperatures,† is rendered considerably more energetic as the temperature rises, until, after a certain relatively high temperature (about 45° C.) is reached, it becomes less intense, and injury to the cells soon results, undoubtedly from damage to the structure of the living substance, owing to the excessive disturbances brought about in its metabolism. Speaking generally, we may fairly say that at temperatures near 0° to 5° C. the respiration is very slow; as the temperature rises the respiratory activity increases, at first slowly, and gradually more and more rapidly, till at 35° to 45° C. it is at its *maximum* intensity; beyond that it rapidly declines, and ceases with the death of the protoplasm at about 50° C.

Light appears to exert little or no effect on the normal process of respiration, unless relatively very intense,‡ when it may possibly promote it; but bright light may accelerate certain processes of oxidation which would otherwise have gone on more slowly.§ This much probably may be said, however: in so far as light influences oxidation processes (other than respiration) in the living cell, the action increases with the intensity of the light.|| As we shall see

* See Sachs, 'Lectures on the Physiology of Plants,' pp. 189—204, 299—308, 552—555, &c., for an introductory general account. The special literature will be noticed as we proceed.

† See Kreussler in 'Landwirthschaftliche Jahrbücher,' vol. 16, 1887, and vol. 17, 1888, for the dependence of respiration on temperature.

‡ Of course referring to ordinary daylight only.

§ See Pfeffer, 'Pflanzenphysiologie,' vol. 1, p. 376, as to possible bearing of this on decomposition of organic acids (Pfeffer, "Über d. Oxydationsvorgänge in leben. Zellen," pp. 454, 469, 472).

|| As to the effect of very intense light, see Pringsheim, "Ueber Lichtwirkung und Chlorophyllfunction in der Planze" ('Jahrb. f. wiss. Bot.,' vol. 12, 1880, pp. 84—93). It should be remarked that Pringsheim not only shows that the action is really due to light- and not heat-rays, but that the more refrangible rays (blue, &c.) are the most active (see pp. 40 and 52). These and others of Pringsheim's observations may be accepted without prejudice as to his theory of assimilation.

later on, there are some other remarkable changes going on in the cell, and connected indirectly with the action of light and respiration, but these do not probably affect the general conclusions just advanced, close as is the connexion between respiration and metabolism generally.

The question now arises as to the quantity of oxygen necessary for respiration, and as to the effects of undue accumulation of the carbon dioxide: it is too long a subject and it is unnecessary to discuss it in detail.* I need only remind you that in the absence of oxygen respiration ceases,† while it is interfered with when the amount of oxygen is much greater per unit of volume than in ordinary air, *i.e.*, when the oxygen is condensed; under ordinary circumstances, however, the free oxygen of the atmosphere amply suffices, provided it can pass readily into the cells and be renewed. Anything of the nature of stagnation must be assumed to impede respiration, whether simply from the accumulation of carbon dioxide, or other products of respiratory activity and consequent metabolism, or because sufficient oxygen-molecules do not pass into the protoplasm in a given time. Since the extremes are not nearly attained in nature, however, I pass by this subject with the remark that in proportion as the intercellular passages or other communications with the atmosphere‡ become blocked by condensed water, for instance, the ventilation of the plant—and therefore its respiration—may suffer§ for the time being simply on account of the slower diffusion of the gases, carbon dioxide and oxygen, from one part of the plant to another.

Coming now to the subject of destructive metabolism, we find that it is affected by external factors; in the first place, by whatever affects respiration, and therefore the foregoing remarks apply to metabolism generally. This is especially so in the case of temperature, and the statements already given may serve broadly with respect to metabolism as a whole. A few details are of importance, however. We have

* For further details, *cf.* the text-books already cited, *e.g.*, Pfeffer, vol. 1, p. 377.

† We are of course not concerned with so-called "intra-molecular" respiration (*cf.* Pfeffer, vol. 1, pp. 370—374).

‡ See Russow, "Zur Kenntniss der Hölzer," &c., in 'Bot. Centralbl.,' 1883 vol. 13, p. 136.

§ It is no uncommon event, even in England, to see the intercellular passages of leaves blocked with suffused water after a cold night, but the phenomenon is much commoner in the tropics, and occurs quite generally in the hill country in Ceylon, for example. As the temperature rises during the morning, the water quickly evaporates and the leaf loses its dark, suffused, limp appearance, and becomes normal. Of course, the phenomenon is due to proportionally more water being absorbed from the relatively warm soil than the cool air can take up. See also Pfeffer, 'Pflanzenphys.,' vol. 1, p. 172, and the literature concerning the ascent of water in plants (collected in Marshall Ward, 'Timber and some of its Diseases,' 1889, pp. 59—141).

spoken of two sets of bodies among the many which are produced during the metabolic processes of the cell—the organic acids and the amide-substances. It appears from the evidence to hand that organic acids are not only formed, but are also subsequently oxidised, in the cell, and it is only to be expected that this process of decomposition of the acids is also promoted by raising the temperature, and conversely,* and such is the case; these acids increase during the night and diminish during the day, and one important factor in the processes is temperature. The *optimum* of increment of organic acids in the plant occurs at somewhat low temperatures†—*e.g.*, about 10° C. to 15° C.; while the *minimum* coincides approximately with that of respiration (near 0° C.), and the acids cease to increase—or, rather, they are decomposed as fast as, or faster than, they are produced—as the temperature rises to 35—40°.

With respect to the effects of light on metabolism, reference may be made as to what has already been said as regards its promoting certain processes of oxidation in the cells,‡ and to what follows on assimilation. The part played by oxygen also has been adverted to; metabolism in the ordinary course of events depends on respiration, and all that affects the latter affects it. In the absence of free oxygen, conditions of intense destructive metabolism are eventually set up, the details of which we need not discuss.§ If plenty of non-nitrogenous food materials are present, the metabolism goes on for some hours as usual, but soon the starving protoplasm undergoes more and more profound changes, resulting eventually in a loss of proteid substances.

It is important to bear in mind that in the cells containing chlorophyll the free acids diminish in daylight, and increase as the light fades and in darkness, no doubt because there is less oxygen in the absence of that set free by the chlorophyll corpuscles; these acids also decrease in proportion as the temperature rises, and increase as it falls. It is also important to be clear in this connexion as to the fact that two processes are going on simultaneously—on the one hand, organic acids are being formed as products of incomplete oxidation in the respiratory processes, and, on the other, they are being further oxidised and decomposed when the temperature is high and the light bright.|| Whether at any given moment the amount of acid present

* See Warburg, 'Unters. aus d. Bot. Inst. zu Tübingen,' vol. 2, Heft 1, 1886, p. 102.

† Warburg, *op. cit.*, pp. 71 and 102, confirming the results obtained by de Vries (literature quoted).

‡ See Pfeffer, 'Ueb. die Oxydationsvorgänge,' &c., pp. 419 and 454.

§ See, however, Palladin, "Ueber Eiweisszersetzung in den Pflanzen bei Abwesenheit von freiem Sauerstoff" ('Ber. d. Deut. Bot. Gesellsch.,' 1888, p. 205).

|| That the connexion with light depends on the access of oxygen set free in

is larger or smaller depends on the resultant action of these processes. Anything which interferes with oxidation promotes the accumulation of organic acids, whereas those changes which lead to increased oxidation in the cells are followed by a decrease of acids.

Now a few words as to growth, and its dependence on external factors. Apart from the thickening of the cell-walls, which comes afterwards and depends on the addition of materials formed by the protoplasm,* the principal phenomenon that concerns us is the extension of the cellulose membranes. This process is promoted by moderately high temperatures, and retarded by low ones and by very high ones,† in accordance with respiration and the general metabolism of the cell; the curves are not quite the same, because respiration begins at temperatures too low for growth, and goes on rising in intensity to temperatures at which growth begins to decline; still the connexion is very close, and the dependence of growth on respiration and metabolism implies this.

Light is usually considered to have a retarding effect on the growth of the cells. Apart from the possibility that there may be a more direct action of light on the extensibility of cell-walls or of cells generally, by its effects on the protoplasm at the spot, one way in which this retarding effect may be brought about is in connexion with the turgidity of the cells. Without concerning ourselves with the general discussion of the whole subject, which would be a very long one, it seems, at least, clear that in the ordinary course of events light exercises some retarding action on growth by extension; what, if any, connexion exists between this phenomenon and the observed diminution of the organic acids in the cells (and we have seen that their turgidity depends on these acids and their salts) in daylight still needs investigation, and the same may be said as regards the influence of temperature in relation to growth and the production of acids. It is customary to regard the retarding action of light on the extensibility of the cell-wall as a complex phenomenon of irritability,‡ and it is by no means certain that such is not the case; meanwhile we simply accept the facts that in ordinary bright light the extension of the growing parts is retarded, that this is connected with diminished turgidity, which in its turn is dependent on the pressure in the sap of substances capable of retaining the

carbon assimilation, and not on any direct action of the rays of light, can hardly be doubted (see Warburg, *op. cit.*, pp. 77—92).

* For details see Strasburger, 'Uber den Bau und Wachsthum der Zellhäute' and "Ueber das Wachsthum vegetabilischer Zellhäute" ('Histologische Beiträge,' No. 2, 1889).

† Sachs, 'Physiology,' pp. 194 and 553.

‡ See Vines, 'Physiology of Plants,' p. 398; but *cf.* also Wortmann, 'Bot. Zeitg.,' 1887, Nos. 48—51, especially col. 808—810.

necessary water. If we accept, with de Vries,* that these substances are chiefly the organic acids and their salts, then we may expect the study of influence of light in promoting the decomposition of organic acids in the plant to give more information on these matters. The same remarks apply with regard to the influence of variations in temperature.

Growth is, of course, impossible without water, and the transpiration current supplies this to the osmotically active cells. In nature, the quantity of water at the disposal of these cells varies enormously, not only with the quantities at the disposal of the root-hairs, but also with the rapidity of the transpiration influenced by the atmosphere. On the whole, given favourable temperature, and other circumstances, growth in length is most active in damp weather, when the quantities of water in the cells are relatively very large ; it is retarded in hot, dry weather, because the loss of water is sufficiently extensive to diminish turgidity.

Passing now to *carbon assimilation*, I come to the subject which offers most interest for our enquiry. Assimilation is also to some extent influenced by temperature, although in a very different manner from respiration ;† and the influence of even large variations in temperature may be masked by the effects of small variations in other factors, especially light. Assimilation takes place at low temperatures whenever respiration is possible, but the temperature curve for assimilation in ordinary bright sunlight is steeper than that for respiration, and at higher temperatures (say, 30° C. and above) where respiration is not yet most active, assimilation is already beginning to decline. In the blackberry, for instance, whereas assimilation is most active at between 29° and 33° C., respiration goes on becoming more and more energetic to 46° C., at and beyond which its effects are of course dangerous to the plant.‡ On the whole, we may conclude that at low temperatures, say, 5° to 10° C , on a bright spring morning, assimilation is relatively more active than respiration, whereas at higher ones—30° to 40°—the reverse is distinctly the case.

The effects of variations in the intensity and kind of light on assimilation have been much studied, and may be summed up generally for our purposes as follows.

With ordinary solar light, as it reaches the plant on a clear day in the open, the activity of assimilation increases nearly in proportion to the intensity§ of the light ; this is usually expressed by saying, the

* 'Unters. über d. mechan. Ursachen d. Zellstreckung,' 1877, and 'Bot. Zeitg.,' 1879, col. 848.

† See Kreussler, 'Landwirthschaftl. Jahrb.,' vol. 16, 1887, and vol. 17, 1888.

‡ See Kreussler, *loc. cit.*, 1887, p. 746.

§ The word must not be pushed too far as to meaning, in the absence of any satis-

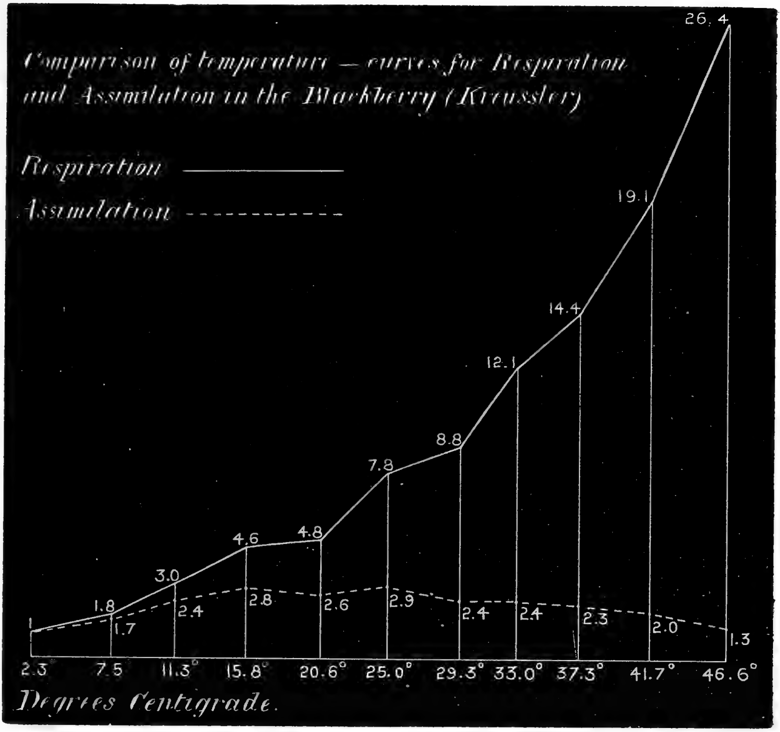


FIG. 6. Diagram constructed to show the comparative effects of equal increments of temperature on respiration and assimilation respectively, according to Kreussler's data. The base line has marked off on it a number of intervals corresponding to so many degrees centigrade, as denoted by the figures; on the ordinates from these points are measured distances corresponding to Kreussler's figures—numbers representing the comparative intensity of the functions in question, if that at the lowest observed temperature is taken as unity.

more light the plant can get, the better. There is evidence to show that, as might be expected, light of great intensity concentrated by means of a lens, &c., on to the assimilating apparatus, produces destructive pathological changes; but we may also infer from everyday experience with shade-plants (*e.g.*, camellias) that the light may be too intense for normal assimilation to go on,* and such is the case.

Another point of importance is the kind of light which reaches the

factory mode of estimating "brightness," "intensity," "quantity," &c., of light (see Sachs, 'Phys.,' pp. 301—302).

* See Famintzin, 'Bull. de l'Académie de St. Pétersbourg,' vol. 26, 1880, col. 296—314. Also Reinke, 'Bot. Zeitg.,' 1883, No. 42, with literature.

plant. I need not remind you that some rays of the solar light, especially some of the less refrangible (orange and red) rays, are more concerned in the process of assimilation than others, and, although we cannot here stop to discuss this matter in detail, I may point out that, as different rays of light are absorbed or reflected in the atmosphere, we may have variations in this connexion of more or less importance to the plant. The experience of photographers shows that the different thicknesses of the atmosphere through which the light has to pass, reflection from a cloud as contrasted with the "blue sky," &c., all exert influence on the composition of the light, and in prolonged cloudy, dull weather or fogs this factor may add its effects to those due to the mere dilution of the light as a whole.

But the fundamental nature of the necessity for a suitable intensity of light of the right composition is best brought out in studying the effects of low intensities of light on the green organs of plants.

As is well known, the general effect of keeping a plant in the dark is to induce a condition known as etiolation.* The whole plant becomes pale yellow or colourless, and has a curiously translucent, watery appearance; the internodes are excessively long, while the leaves, on the contrary, are usually small and crumpled. Closer investigation shows that each cell of the internodes is abnormally elongated, its cell-wall thinner than usual, and its chlorophyll corpuscles small and wanting the green chlorophyll. If we examine the vascular bundles, they are found to be deficient in firmness, because the substances which normally go to thicken their walls have not been forthcoming.

Everything about the etiolated shoot indicates tenderness, and as a matter of fact such shoots are very ill-adapted to withstand the ordinary exigencies of plant life. Undoubtedly the chief cause for this weak condition is the absence of the light necessary for the purposes of assimilation; the carbon dioxide may be present, and even the fully green chlorophyll could be developed by a few hours' exposure to feeble light, but these do not suffice for the construction of the materials such as glucose, starch, &c., necessary to enable the protoplasm to keep the tissues normal.

Nevertheless, the other functions of the cell are being carried on with remorseless pertinacity. The oxygen of the air enters the protoplasm, establishes its usual combinations, and carbon dioxide and water are given off to the air. The chemical changes known collectively as metabolism proceed, and result in the addition of substances to the cell-sap which were not previously there. To an extent more marked than ever before, the turgid cells may be elongating, and this

* See Sachs ('Bot. Zeitg.,' 1863, supplement, and 1865, col. 117, &c.); G. Kraus ('Jahrb. f. wiss. Bot.,' vol. 7, p. 209; Godlewski ('Bot. Zeitg.,' 1879, col. 81); de Vries ('Bot. Zeitg.,' 1879, col. 852); Godlewsk (Biol. Centralbl.,' 1889).

brings us to note that the key to the condition of affairs is the fact that the dry weight of the etiolated shoot is decreasing: every molecule of carbon dioxide which comes away lessens the dry organic substance of the plant, and no restoration of such substance is possible in the absence of light.

In other words, then, the etiolated plant is growing to death, at the expense of what organic carbon-compounds it possessed at the beginning.

Assimilation is, of course, profoundly affected, like every metabolic process, according to the relative amounts of oxygen and carbon dioxide in the air, and although it never happens in nature that the extremes are approached, nevertheless experiments on this subject have led to interesting results.*

The quantity of water present in the plant and its atmosphere and the rapidity of the transpiration current undoubtedly affect the process of assimilation in a high degree. Not only is water needed for the molecular processes concerned in the act of assimilation, and not only does the supply of materials to the protoplasm depend mainly on the transpiration current, but, as we have seen, the aëration of the intercellular passages, and consequently the movements of gases generally are affected.

It has long been known that the quantities of carbon dioxide absorbed, and of oxygen evolved, in the process of assimilation, vary with the age of the leaf or other organ concerned, and Kreussler has shown that one reason for these variations is the quantity of water present in the tissues at the time. In fact, an essential cause of variations in assimilation exists in the differences in the water contents of the tissues,† and it is no doubt largely due to the want of water that older leaves assimilate so unequally—they are unable to rapidly restore the equilibrium between losses and gains when it is seriously disturbed.

As for transpiration itself, and all the movements of water correlated with it, it is well known that the various factors of the environment affect it profoundly.

Apart from the more obvious relations‡ between transpiration and the temperature of the atmosphere, and the quantity of aqueous

* See Godlewski, "Abhängigkeit der Stärkebildung in den Chlorophyllkörnern von dem Kohlensäuregehalt der Luft" ('Flora,' 1873, p. 378); also in 'Arb. des Bot. Inst. in Würzburg,' 1873, vol. 1, p. 343. Further, Pringsheim, "Ueber die Abhängigkeit der Assimilation grüner Zellen von ihrer Sauerstoffathmung," &c. ('Sitzungsber. d. Kgl. Preuss. Akad. der Wiss. zu Berlin,' 1887, No. 38, pp. 763—777).

† Kreussler, "Beobachtungen über die Kohlensäure-Aufnahme und -Ausgabe der Pflanzen," II ('Landwirthsch. Jahrb.,' vol. 16, 1887, especially pp. 728—30).

‡ See the text-books referred to, especially Pfeffer, 'Pflanzenphysiologie,' vol. 1, pp. 146—150.

vapour in it, it must be borne in mind that many events concur in promoting or retarding it. The stomata, for instance, open widely in bright sunshine and close in the dark,* a matter of great importance in controlling transpiration, as must be concluded from the researches of Garreau and von Höhnel.† Other effects are traceable to the influence of the wind shaking the plant, and to the quantities of mineral salts, &c., in the soil, but it would carry me too far to discuss further instances.

The principal effects of obstructed transpiration may be shortly compared with those due to want of light—the watery tissues are strikingly like those of an etiolated plant, and we may look upon a shoot growing in a saturated atmosphere as presenting all the chief features of one growing in darkness.‡ Its cells are extremely turgid, with watery, soft, thin walls, and acid cell-sap; its vascular bundles feebly developed and hardly lignified; and, as before, it is ill adapted to withstand the exigencies of the ordinary environment.

All such plants or organs are, so to speak, in a permanently young condition.

The Effect of the Preceding Variations in "Predisposing" the Host to Disease.

If we put together the results of the preceding discussion, it is evident that a plant may vary within very wide limits of the condition we term health. No doubt this needs no proof to the minds of most of my hearers, but the point I wish to emphasise is that, in some of its deviations from the normal, the plant offers conditions to an attacking parasite which may be at one time favourable, at another not.

Suppose the case of a herbaceous plant growing under the following circumstances in July: the temperature has been high, and the daily supply of solar light abundant during the previous four or five weeks, and everything has been going on admirably, so far. Suddenly the weather changes—the temperature falls, rain sets in, and for many days heavy clouds obscure the sun. If this markedly different, dull, cold weather continues, we may have the following condition of affairs more or less realised, as is well known to those who observe cultivated plants closely.

Transpiration being lowered in activity, the whole plant tends more and more to be suffused with water; the stomata are nearly closed,

* See Sachs, 'Lectures on the Physiology of Plants,' pp. 248—250, and Strasburger, 'Das Botanische Practicum,' 2nd ed., 1887, pp. 88—90.

† See Pfeffer, 'Pflanzen-Physiologie,' vol. 1, p. 144.

‡ Vesque and Vie[illegible], "Influence du Milieu sur les Végétaux" ('Ann. d. Sci. Nat.,' 6 Sér. (*Botanique*), vol. 12, 1881, p. 167).

the cell-walls bounding the inter-cellular passages and the air in the passages themselves are thoroughly saturated with water and aqueous vapour respectively, and the movements of gases must be retarded accordingly, turgescence is promoted, and the water contents accumulate to a *maximum*, owing to the disturbance of equilibrium between the amounts absorbed by the active roots in the relatively warm soil and those passing off into the cold damp air; much more water is absorbed by the roots in the relatively warm soil than passes off as vapour in equal periods of time. An enhanced wateriness of the whole plant, then, is one result.

But the low temperature, feeble light, and partially blocked ventilation system have for a consequence a depression of respiratory activity and the absorption of oxygen generally. Enough oxygen gas finds its way slowly into the cells to keep the life-processes going, of course, but not enough to complete the oxidations and decompose the organic acids, at the prevailing low temperature, so rapidly as before,* and thus another consequence is a tendency to the accumulation of organic acids. According to de Vries, however, the increase of organic acids must make itself effective in enhancing the turgidity of the cells, and no doubt it does so to a certain extent; beyond a certain point, however, it is more likely to increase the permeability of the protoplasm,† and we may even suppose small quantities of the acids to filter out even to the watery cell-walls.‡

Partly due to the low temperature and the depressed gas-interchange, but far more owing to the feeble light, the process of assimilation will be less active than previously. This will not be immediately felt if, as will probably be the case, there are large quantities of temporary reserves in the leaves and internodes; but it may react indirectly on the processes of oxidation and respiration, inasmuch as less free oxygen is evolved in the cells than would be the case in bright weather. As the temporary stores of starch disappear, however, the cells become more and more surcharged with glucose, together with organic acids, and it depends on several circumstances, especially on how rapidly growth is going on (*e.g.*, in the parts below ground), whether this glucose in solution passes away, or is used up slowly or rapidly; if it cannot move, or only extremely slowly, then we have the case of tissues surcharged with water containing organic acids and glucose in solution. It may be surmised

* See Warburg, *op. cit.*, especially pp. 73—77, and 126.

† See Pfeffer, "Ueber Aufnahme von Anilinfarben in lebenden Zellen," in 'Unters. a. d. Bot. Inst. zu Tübingen,' vol. 2, 1886, pp. 296 and 329, for proof that dilute acids can traverse without permanently injuring the protoplasm.

‡ Pfeffer showed, for instance, that methyl-orange, after being taken up in the living cell and held there by the protoplasm, can be made to diffuse out again if a little citric acid is imbibed ("Üb. Aufnahme," &c., *op. cit.*, p. 293).

that the increased amount of organic acids is favourable rather than otherwise to the ferment processes which lead to the conversion of the starch into glucoses.* How far the protoplasm will allow the watery solution of glucose to escape, owing to its increased permeability, cannot be determined, but it is at least probable that some may reach the cell-walls. In any case, we have the cells flooded with a dilute solution of organic acids and glucose, and the controlling protoplasm becoming less and less capable of retaining the excess.

The turgid condition of the cells, and the diminished intensity of the light,† will favour growth, and, in spite of a comparatively low temperature, the organs may be extending more or less rapidly or slowly. If so, the tendency will be for the very watery cell-walls to become relatively thinner than usual, as well as watery, because the ill-nourished protoplasm does not add to the substance of the wall in proportion. This being so, we have the case of thinner, more watery cell-walls acting as the only mechanical protection between a possible fungus and the cell contents.

But this is by no means all that has to be considered, when the conditions remain as above described. Sooner or later the glucose begins to fail, either because it has been directly employed for the support of the metabolic processes in the protoplasm in the immediate neighbourhood, or (less probably) because it has been re-converted into starch,‡ or other reserve carbohydrates by the leucoplasts in the cells of the roots, tubers, &c., at a distance. Now, as soon as a want of carbohydrates makes itself evident in the destructive metabolic processes accompanying growth, the accumulation of substances like asparagin, leucin, &c., is apt to occur,§ as products of the decomposition of the proteids. Under more normal conditions, as we have seen, these amide-bodies would be worked up again with carbohydrates into new constituents of living protoplasm, but they now begin to accumulate.

The net result of the foregoing changes amounts, shortly put, to the following:—Under certain circumstances the parenchymatous tissues of the living plant may be in a peculiarly tender, watery condition, where the cell-walls are thinner and softer, the protoplasm

* Baranetzky, 'Die Stärkeumbildenden Fermente,' 1878; Brown and Heron, 'Journ. Chem. Society,' 1879; Detmer, 'Das Pflanzenphysiologische Praktikum,' 1888, p. 198.

† So far as the *composition* of the light is altered, it will probably favour growth, because the more refrangible rays are fewer when the light has to traverse a thick atmosphere.

‡ See Schimper, 'Bot. Zeitg.,' 1880, col. 881; and A. Meyer, 'Bot. Zeitg.,' 1880, Nos. 51 and 52.

§ See Palladin, "Ueber Eiweisszersetzung in den Pflanzen" ('Ber. d. Deutsch. Bot. Gesellsch.,' 1886, p. 205); and for older literature, Pfeffer, 'Pflanzenphysiologie,' vol. 1, pp. 298—301, and further literature quoted.

is more permeable and less resistant, and the cell-sap contains a larger proportion of organic acids, glucose, and soluble nitrogenous materials than usual. When the external conditions become more favourable—the temperature higher, the air drier, and the sunlight more powerful—increased transpiration and respiration lead to more normal metabolic activity, for which energetic assimilation provides the materials. Of course, all kinds of combinations are possible in detail, but when dull, cold, wet weather prevails for some time, after a period of bright, hot, and dry weather in the early summer, we are very apt to have herbaceous plants in such a condition as that sketched.

This being so, I have now to show how the chances of a suitable fungus are increased, if it happens to start its parasitic life on such a host in such a condition.

Botrytis *and other Fungi as Agents of Disease, and their Dependence on the Condition of the Host Plant.*

Let me first proceed to call your attention to a parasitic disease of a very extraordinary kind, though caused by a fungus belonging to a well-known and widely-spread family. This disease, and the fungus in question, may be met with in nearly every garden and greenhouse all the year round, and is quite common in the open fields and lanes of this country and elsewhere in Europe. In the form generally met with, the fungus has been placed in a separate genus known as *Botrytis*, though, in the few cases that have been thoroughly worked out, it has been proved that the mould-like *Botrytis* is only the conidial form of certain higher ascomycetous fungi belonging to the *Pezizas*, and which agree in developing sclerotia. As we are not concerned with the details of the whole life-history of this group, I shall purposely avoid further reference to the higher stages of development, confining our attention chiefly to the *Botrytis* stage.*

On dead and dying leaves, twigs, fruits, &c., of plants from all parts of the world, in the open and in greenhouses, in Europe and elsewhere, there is often to be observed an ashen-grey mould, superficially not unlike the *Phytophthora* of the potato-disease. It appears under various slightly different aspects as regards the shade of colour, the length and degree of branching of the conidiophores, and the size and shape of the conidia, and many different species have been figured and described, some good, many bad, according to the variations in colour, size, &c., referred to, and the substratum on which the mould is found growing.

It sometimes happens, however, that this same mould is found

* For further details as to the morphology, &c., *cf.* de Bary, 'Comp. Morph. and Biology of the Fungi,' &c., especially under the heading *Peziza Fuckeliana*.

Fig. 7. A dead leaf infested with moulds, especially with *Botrytis*, as shown on the grey patches. (Natural size.)

spreading more or less rapidly from dead and dying parts of a plant to the assumed healthy organs, and it has been customary to look upon this as a secondary phenomenon due to the "dying-off" of the adjoining parts, the fungus spreading to them as they died. No one questioned the saprophytic nature of the *Botrytis*,* and so the matter stood for a long time. Gradually, however, it came to light that various forms of this *Botrytis* appear as phases in the life-history of certain sclerotium-bearing *Pezizas* which were associated in a manner suspicious, to say the least, with epidemic diseases of rape,† clover,‡ hemp,§ onions,|| hyacinths¶ (also *Scilla*, *Narcissus*, *Anemone*, &c.), balsams,** *Carex*, rice, and many other plants.

Further, this mould was found causally associated with the rotting

* Of course I am referring to the modern definition of the genus *Botrytis*, after its separation from the totally different Peronosporeæ (see 'Annals of Botany,' vol. 2, p. 357).

† See Coemans in 'Bull. Acad. Roy. de Belgique,' Sér. 2, vol. 9, 1860, p. 62; and Frank, 'Krankheiten der Pflanzen,' 1880, pp. 531—537.

‡ Kühn, 'Hedwigia,' 1870, No. 4, p. 50; Sorauer, 'Pflanzenkrankheiten,' vol. 2, 1886, pp. 283—288; Rehm, 'Die Entwickelungsgeschichte eines die Kleearten zerstörenden Pilzes,' Göttingen, 1872.

§ Tichomiroff, in 'Bull. Soc. Nat. de Moscou,' 1868, 2 (see Hoffmann's 'Mykol. Berichte,' 1870, p. 42).

|| See Frank, *op. cit.*, p. 540, and Sorauer, 'Oesterr. Landwirthsch. Wochenbl.,' 1876, p. 147, and 'Pflanzenkrankheiten,' vol. 2, p. 294.

¶ Meyen, 'Pflanzenpathologie,' 1841, pp. 164—172; and Wakker, in 'Bot. Centralbl.,' 1883, vol. 14, p. 316.

** Frank, *loc. cit.*, p. 544.

of many fruits, such as pears and apples,* grapes,† cranberries,‡ &c., and on chestnuts.§ In short, even the forms of *Botrytis* which were most persistently regarded as saprophytic have now been shown to enter living plants and cause parasitic diseases in them,‖ and complaints of such epidemics are occasionally heard from various parts, as a rule, however, the disease is sporadic, and I now proceed to describe its symptoms.

FIG. 8. A bunch of "mouldy" grapes infested with *Botrytis cinerea.* The ravages of the fungus cause the skin to rupture, and the fruits to shrivel from loss of water; other changes in the substance of the contents are referred to in the text. Patches of the conidiophores are seen on the exterior (Müller-Thurgau).

Small reddish-brown spots appear on the leaves, pedicels, ripening fruit, or other organ attacked; these enlarge and spread, and the parts turn brown, shrivel up, and rot off or dry up, according to the state of the weather. In some cases the whole plant gradually turns

* Sorauer, 'Pflanzenkrankheiten,' vol. 2, p. 298.

† See especially Müller-Thurgau, in Thiel's 'Landwirthsch. Jahrb.,' vol. 17, 1888, pp. 83—160, on "Edelfäule."

‡ See especially Woronin, 'Mém. de l'Acad. de St. Pétersb.,' vol. 36, No. 6, 1888.

§ Kissling, 'Zur Biologie der *Botrytis cinerea,*' Bern, 1889, p. 14 (where also the literature is collected).

‖ *E.g.*, *B. cinerea*, the conidiophores of *Peziza Fuckeliana* (see de Bary, 'Comp. Morph. and Biol. of Fungi,' p. 380), is now known to be capable of producing epidemic diseases in vines, gentians, &c.

yellow and dies, more often only a part of it goes,* and in many cases the disease is confined to individual organs—leaves, flower-buds, fruit, &c., as the case may be. When the disease occurs amongst stored chestnuts, carrots, parsnips, &c., the tissues become speckled, and in many cases this spreads till they are rotten throughout; and similarly with stored bulbs, corms, and tubers, &c.

Wherever the disease is rampant we find the colourless, septate, branched fungus mycelium in the dead and dying tissues, and usually emitting hyphæ, which grow into the damp air and bear the conidia in abundance. In some cases, however, these aërial conidiophores have not been observed,† and the habit of producing them appears to be lost, though in every other respect the behaviour of the mycelium is the same in all the cases thoroughly examined.

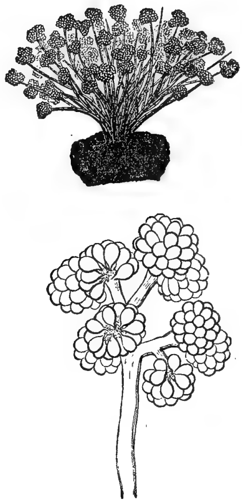


FIG. 9. *Botrytis cinerea.* The upper figure represents a tuft of the conidiophores breaking through the epidermis of a grape (magnified); the lower one is one of the conidiophores still more highly magnified.

Some very remarkable facts have come to light during the last few years concerning this mycelium and the conidia; and as all the species or forms which have been thoroughly examined agree essentially in their physiological behaviour, I need no longer trouble you

* A curious fact is sometimes observed—the small brown spot suddenly ceases to spread, and the hyphæ may be found in it in a dried-up, dormant condition for weeks. See also 'Annals of Botany,' vol. 2, 1888, p. 356, and figs. 51—54.

† *E.g.*, in de Bary's *Peziza Sclerotiorum* (Lib.). See "*Sclerotinien und Sclerotinien-Krankheiten*" ('Bot. Zeitg.,' 1886, col. 424).

with references to any special forms, excepting in so far as the citation of authorities necessitates this.

In the first place, the mycelium and conidia are not only capable of growing and flourishing in artificial nutritive media, but they often refuse to do otherwise—at least while young. If the conidia are sown in such media as the juice of grapes or other fruits, or in solutions containing an organic acid, sugar, asparagin, and traces of mineral salts, enormous cultures may be made for weeks, and millions of new conidia, sclerotia, &c., obtained, provided certain conditions are fulfilled. Among these conditions are the following:—The temperature must not be high, and may be relatively low (best about 15° C.); the solution must not be alkaline or neutral, but should be somewhat acid;* sugar of some kind—and preferably a glucose—must be present; and the nitrogenous materials may be offered as asparagin or peptone with advantage.

It will be noted that just those external climatic conditions which we have seen to be disturbing to the well-being of the green host-plant are either favourable to the fungi we are concerned with, or are at any rate not in the least inimical to their development.

Thus the oxygen respiration of the fungus goes on at all temperatures from 0° C. to 30° C. and higher, and, although we still want information as to details, experiments have shown that the mycelia flourish at temperatures considerably below the *optimum* for higher plants.†

Moreover light, so indispensable for the carbon assimilation of the green host, is absolutely unnecessary for the development of the fungus.‡

Then, again, the dull, damp weather and saturated atmosphere, so injurious to higher vegetation if prolonged, because they entail interference with the normal performance of various correlated functions, as we have seen, and render the plant tender in all respects, are distinctly favourable to the development of these fungi.

Consequently the very set of external circumstances which make the host-plant least able to withstand the entry and devastation of a parasitic fungus like *Botrytis*, at the same time favour the development of the fungus itself.

As already said, it had long been assumed that these forms of *Botrytis* are saprophytes, and the ease with which they may be culti-

* See Marshall Ward, "A Lily Disease" ('Annals of Botany,' vol. 2, 1888, p. 334); also *cf.* de Bary ('Bot. Zeitg.,' 1886, col. 400).

† See also Hoffmann ('Jahrb. f. wiss. Bot.,' vol. 2, 1860, p. 267) and Zopf. "Encyklopædie der Naturwiss." (Schenk's 'Handbuch,' vol. 4, 1889, pp. 471—472).

‡ According to Klein ('Bot. Zeitung,' 1885, col. 6), the conidia of *Botrytis cinerea* are only developed in the darkness of night, but this is certainly not the case with other species.

vated in artificial solutions, as above, tended to support that view: moreover, many attempts to directly infect living plants with the conidia failed—the conidia, if merely placed in a drop of water on a healthy leaf, simply germinated and died, and very often nothing more came of it. Nevertheless, odd instances of infection were recorded here and there, and the whole matter became a great puzzle,* until several points of startling importance came to light.

In the first place it turned out that, although the germinal tubes of certain of the *Peziza*-forms could not penetrate into the living leaf of the host directly—whereas they plunged forthwith into the tissues of a dead organ†—nevertheless the *mycelium* developed from such spores, provided it was vigorous and well nourished by previous culture as a saprophyte, could do so, but in many cases only provided the tissues of the host were in a favourable condition. This last proviso was found to be necessary, because in some cases the mycelium easily infected young growing internodes, &c., but could not penetrate into the more fully developed older parts of the same plant.‡ This threw some light on the curiously capricious behaviour of the fungus in green-houses, where seedlings, cuttings, young internodes, &c., were often attacked and destroyed, while older parts escaped, though without any regularity of behaviour.

The key to the mystery appeared to be offered when it was discovered that the invigorated mycelium, well nourished by cultivation in a solution such as that mentioned above, excretes a ferment which possesses the power of swelling and dissolving cellulose, and that this ferment is formed at the tips of the hyphæ,§ and thus enables them to enter the cell-walls, as they were actually seen to do. It becomes intelligible now why these hyphæ sometimes can and sometimes cannot quickly enter the cell-walls of a plant: when the cell-walls are thin and watery, and especially if small quantities of organic acids are present, the fungus hyphæ can easily attack and dissolve them, but in cases where they are thick and tough, owing to paucity in water and no traces of acids, the hypha has no chance. Just such differences as these would occur in the case of young and old organs respectively, or of partially etiolated or thoroughly matured tissues respectively.

But in addition to piercing the walls, and at first living in the

* We shall see that the occasional infection depends on (1) condition of host, (2) whether any soluble food-materials pass from the leaf into the drops of water, and (3) the state of the conidia.

† See de Bary, "Ueber einige Sclerotinien und Sclerotinien-Krankheiten" ('Bot. Zeitung,' 1886, col. 410).

‡ See de Bary, 'Bot. Zeitg.,' 1886, col. 440—441.

§ See Marshall Ward, "A Lily Disease" ('Annals of Botany,' vol. 2, pp. 339—343).

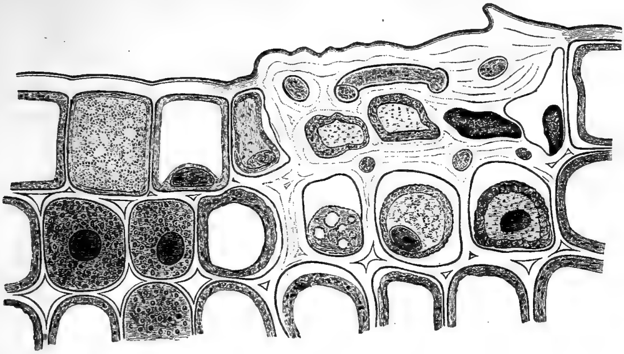


FIG. 10. Portion of a transverse section through an infection-spot in the tissues of snowdrop (such as that at *a* in fig. 11), showing the swollen cell-walls with hyphæ of *Botrytis* in them. The cell-contents also show changes; the protoplasm contracts, dies, and turns brown, and stains less and less readily the further the changes proceed; the cell-sap escapes and suffuses the cell-walls; the nucleus is the last to succumb. The above changes are exhibited by cells some distance away from the hyphæ, and are the less pronounced the further away the cells are. The colour reactions are of course not reproduced. (Very highly magnified.)

cellulose substance,* the hyphæ also excrete a soluble substance which kills the protoplasm (with which they are not in contact) of the cells in the immediate neighbourhood: whether this substance is a separate zymase, or whether it is the same soluble ferment as that which swells the cellulose, is not clear, or whether the protoplasm simply dies after excessive plasmolysis due to water passing into the swollen walls, but it is clear that some such poisonous action is exerted at a little distance from the tip of the hypha, and therefore by means of a soluble poison or zymase of some kind. It is difficult to decide what this poison is, and the following questions arise:—first, Is the poison the same zymase as that which causes the swelling and solution of the cellulose? This must be denied provisionally, at any rate, because if sections of the tissues are put into solutions containing extracts of the mycelium which have been previously boiled for a minute or two, the protoplasm contracts and dies much as before, though the cellulose walls no longer swell as before because the zymase has been killed by the boiling. This experiment is not quite conclusive, because the contraction of the protoplasm may be due to the action of bodies in the boiled extract which did not exist in the freshly expressed liquid.†

* See 'Annals of Botany,' vol. 2, p. 356 and figs. 55 and 56.

† De Bary inclined to the belief in a special ferment in the case of his *Peziza* (*loc. cit.*, pp. 418—420), but admitted that he had not proved the point either way.

The next question which arises is—Is there any definite body in the extract that could kill the protoplasm, and which would not be destroyed by the short boiling? The answer to this question is simple: the hyphæ of the fungus develop large quantities of oxalic acid* in the substratum, and this is a substance which is peculiarly poisonous to the living protoplasm of higher plants† if present in any large quantity. In the normal cells of plants rich in salts of oxalic acid (*Oxalis*, *Begonia*, &c.) I need only remind you that the acid is not in the protoplast, but is kept strictly isolated from it by the vacuole wall, as is clear from the researches of Pfeffer and De Vries.

It is at least conceivable, therefore, that the hyphæ kill the cells by flooding the protoplasm with oxalic acid; but it is not certain that they do not excrete a more subtle poison of the nature of a ferment.

In any case, it is a significant fact that the hyphæ kill the cells by emitting some soluble poison which causes the protoplasm to collapse, and then to turn brown, clearly because it destroys its power of retaining and restraining the sap in the sap-cavity; the latter therefore escapes through the now permeable dead or dying protoplasm, and owing to its acid contents, chromogenes, &c., stains it and the cell-walls brown as the oxygen of the air enters into combination.

There is thus, from the very first, a struggle between the hypha of the fungus and the cells of the host; the hypha is in the position of an attacking party, which has to overcome, first the outworks, in the shape of cuticle and cell-wall, and secondly the real fighting force—the protoplasm.

I take it that the attacking hypha (invigorated by previous culture, as said) excretes various zymase-like substances formed in its metabolism; one of these succeeds in overcoming the resistance afforded by the cuticle‡ and then the cell-wall is penetrated: the partially victorious hypha then advances in the cell-wall, and is nourished by the cellulose which it goes on dissolving, and under its changed conditions of life excretes in increasing quantities yet another zymase or some kind of poison which diffuses to the protoplasm. Now comes the real tug of war—so long as the outer layer of the protoplasm (the ectoplasm) is in a position to refuse access to, or in any way to destroy, the poison, the rest of the protoplast remains impermeable, and the hyphæ keep to the cell-

* De Bary observed the same in the case of *Peziza sclerotiorum* (*op. cit.*, pp. 399—403), and it is a common phenomenon in fungi. See, *e.g.*, Zopf (Schenk's 'Handb.,' vol. 4, 1889, p. 454).

† See de Vries, "Plasmolytische Studien über die Wand der Vacuolen" ('Pringsheim, Jahrb.,' vol. 16, 1885, pp. 565—6).

‡ Not impossibly, different zymases are concerned. See Wortmann ('Zeitsch. für Physiol. Chemi6,' vol. 6, 1882, pp. 287—329, especially pp. 321—329).

walls; but as soon as the protoplasm of a cell succumbs, it signifies its defeat by collapsing, and then its own more or less acid cell sap filters through to the walls and hyphæ. If this view is correct, and the evidence supports it entirely, it is clear that any variations in the host-plant which lead to weakening the outposts—cuticle and cell-wall—or diminishing the fighting power of the protoplast, increase the advantages of the hypha to a corresponding extent; and we have seen that such variations exist when circumstances cause the cells to become more watery and turgid, the cell-walls thinner and softer, and so forth. But, no doubt, the most important event is the lowering of the resisting power of the protoplasm, as must happen whenever external changes—such as low temperature, murky weather and a saturated atmosphere, &c.—combine in lessening the activity of respiration and assimilation, and consequently bring about the accumulation and possible filtration of organic acids, glucose, asparagin, &c.; for, in the first place, the lowered metabolism means less resisting power, no matter what hypothesis we adopt; and, secondly, the organic acids themselves prove an internal source of weakness if they become too abundant, and filter through the partially disorganised protoplasm. The protoplasm, then, has its powers diminished or destroyed by the accumulation and inhibitory action of products of its own activity.

Fortunately, however, we have something more than the above evidence, strong as it is, to support this view. Müller-Thurgau found, in the case of grapes devastated by *Botrytis cinerea*,* that the *Botrytis* mycelium lives on the sugar, acids, and soluble nitrogenous substances of the living cells; but he also discovered, by means of numerous comparative analyses, that the mycelium consumes especially the organic acids, the sugars to a less extent, and the soluble nitrogenous matters were all converted into insoluble nitrogenous substances.† No doubt some of the destruction of the acids is to be put down to direct oxidation, but much is due to assimilation by the fungus. Similar events were found to occur when the *Botrytis* was cultivated as a saprophyte on the juice of grapes.

It is, however, not difficult to give experimental proof of the accuracy of the statements that the entrance of the hypha into the cell is dependent on the condition of the protoplasm.

If the mycelium of one of these fungi is placed on an uninjured

* Thiel's 'Landwirthsch. Jahrb.,' 1888, pp. 83—160.

† *Penicillium* behaved very differently: it took the sugars in greater proportion than the acids, and left the juices more acid than before. *Botrytis*, on the other hand, left the juices less acid than before, and more concentrated owing to the evaporation of water from the injured grapes, and it is interesting to note that these diseased (so-called *edelfäule*) grapes yield the best and sweetest wines of the Rhine district.

living carrot, or turnip,* and the whole placed in a moist atmosphere, &c., the hyphæ do not at first enter the tissues as above described, but form a dense mycelium on the surface, and branches from this slowly penetrate into the interior, producing the symptoms referred to. If the carrot or turnip is first submerged for half a minute into boiling water, however, the hyphæ plunge into the outer cells (the protoplasm of which has been killed by the hot water) at once. These facts are easily explained when we recognise that the hot water causes plasmolysis of the cells, and escape of the sap into the intercellular spaces, cell-walls, &c.; in short, destroys the fighting power of the protoplasm against the hyphæ. Moreover, the latter are invigorated by their saprophytic nutrition, and are able to excrete such quantities of ferment that the still living cells deeper in the tissues are unable to withstand the attack.

Equally conclusive is the following experiment:—

A mature firm shoot of a Petunia was infected with the mycelium, and the hyphæ penetrated into the cortex about 1 cm., and then grew no further; evidently because the cell-walls were thick, and their protoplasm disposed of the poisonous zymase as fast as it reached them. When the infection was made on slightly etiolated, rapidly growing shoots, however, the fungus entered at once, and destroyed the entire shoot off-hand.† This is explained by the thinner, watery cell-walls, and the less vigorous protoplasm, more acid cell-sap, and so forth, of the latter offering less resistance to the zymases or poison execreted by the hyphæ.

Another excellent case is the following. During the very wet, cold, and dull weather of the summer of 1888, plants suffered a good deal from such diseases as we are discussing, and the white lily-buds were utterly destroyed in my neighbourhood by an epidemic of *Botrytis*,‡ aggravated by the low rate of transpiration, and consequently retarded respiration and metabolism, and the diminished assimilation leading to paucity of carbohydrates. The cell-walls were thin and watery, the sap unduly charged with acids, &c., and the protoplasm of the cells less capable of dealing with the poison emitted in larger and larger quantities by the hyphæ of the invading fungus.§ It was a very easy matter to directly infect the tissues of these lilies at the time mentioned, but considerably more difficult to do so when

* As a rule, roots are less acid than other organs, and inflorescences more so than leaves, which again are more acid than the stem (G. Krauss, 'Ueber die Wasservertheilung in der Pflanze, IV, Die Acidität d. Zellsaftes,' 1885; also Warburg, *loc. cit.* p. 116).

† De Bary, 'Bot. Zeitung,' 1886, col. 440—441.

‡ See 'Annals of Botany,' vol. 2, 1888, pp. 319—376.

§ Warburg showed that the leaves of *Lilium candidum* contain more acid when the temperature is lowered (*op. cit.*, p. 140).

the weather improved; and I have noticed the same fact in other cases as well.

Invigoration of Mycelium and Conidia by Saprophytic Mode of Life, and Differences in Behaviour of Successive Generations.

We have seen from the foregoing that the relations of the host to the parasite may depend very much on the condition of the former, as induced by the complex action of the environment. I have now to show you that the variations from a normal which culminate in an epidemic are not confined to the host; but that the parasite also exhibits phenomena leading to the same result.

That the mode of nutrition influences the vigour and size, &c., of a fungus is a fact well known; but it is a comparatively recent discovery that certain fungi, usually saprophytic in their habits, may be educated as it were to parasitism.* Thus, *Penicillium glaucum*, usually regarded as the type of a saprophyte, causes rotting in fruits, bulbs, &c., when its spores penetrate into a wound in the living organ;† and many other fungi usually met with as saprophytes are capable of assuming a parasitic mode of life if opportunity arises,‡ *e.g.*, species of *Mucor, Pythium, Nectria, Agaricus*, &c.

The most instructive of all is the genus *Botrytis*, however, for it is apparently possible to carry the process of "educating" this saprophyte to habits of parasitism much further than in any other cases known.

It was pointed out as early as 1874, by Zimmermann,§ that *Botrytis cinerea*, long known as a common saprophyte on fallen dead vine leaves, passes from the rotting *débris* on the ground to the healthy living leaves of several plants and develops spots on them; and the same fungus has been found as a parasite on the male inflorescences of junipers, thujas, and other Conifers,‖ as well as elsewhere. But a still better case than any of these is the occurrence of a severe epidemic on the gentians in the Jura during the wet summer of 1888.¶ The infection of the plants occurred in the young parts

* The converse is also true to a certain extent. See B. Meyer "Ueber die Entwicklung einiger parasitischen Pilze bei saprophytischer Ernährung" ('Landw. Jahrb.,' 1888, vol. 17); and Brefeld, 'Botan. Unters. ü. Hefenpilze,' Part V, 1883.

† See Sorauer, 'Pflanzenkrankheiten,' vol. 2, p. 92; Müller-Thurgau (*op. cit.*) says *Penicillium* causes a speckling of living grapes.

‡ See de Bary, 'Comp. Morph. and Biol. of the Fungi,' pp. 379—380.

§ "Ueber verschiedene Pflanzenkrankheiten" ('Hamburger Garten- und Blumenzeitung,' 1874).

‖ Klein, 'Verhandl. d. Zool.-Bot. Gesellsch. zu Wien' (vol. 20, p. 547), and Sorokin, 'Mykologische Skizzen' (Charkow, 1871).

¶ Kissling, 'Zur Biologie der *Botrytis cinerea*' (Dresden, 1889, p. 6). It is worth notice that this epidemic occurred in the same dull, cold, damp summer (1888) as the one on lilies in this country.

of the flowers, especially the stigmas and anthers, by means of spores. After growing outside the organs for some time, the hyphæ—now invigorated by their saprophytic nutrition—were able to enter other tissues, *e.g.*, those of the leaves, pedicels, &c. Experiments were then tried with *Echeveria metallica** with complete success, and it may be remarked that this disease is very common in a sporadic form on Crassulacæ in green-houses. Infections of *Lilium* were also successful, and *Hemerocallis flava* was destroyed with extraordinary rapidity. Many other plants were also infected successfully.

In all these cases the spores only infected (directly) the most delicate or least protected parts of the flower, but the resulting mycelium when invigorated by its growth in the dead tissues was capable of directly infecting the ordinary tissues of plants.

It will be remembered that de Bary came to a similar result with his experiments,† and I have observed the same phenomenon over and over again with several of these forms. There is one, for instance, which sometimes cases great havoc among snowdrops in the early spring, and I found that the infection occurred especially by means of small invigorated mycelia developed from spores which germinated on the dead tissue of the sheaths at the base of the leaves; these hyphæ easily penetrated into the etiolated bases of the leaves and young flower-buds, especially when the plants were partially buried in snow.‡ Similarly with onions, hyacinths, and other plants; and similarly in every greenhouse on plants too far from the light, and often in store cellars on etiolated geraniums, calceolarias, and other plants put by for the winter.

But a still more remarkable proof of the influence of nutrition on the fungus is shown in the recent discovery that the conidia of successive generations of the *Botrytis* have different powers of infection.

It has already been pointed out that the conidia may or may not directly infect the tissues, and that one set of events affecting this is the condition of the tissues themselves: another, however, is the kind of food-materials on which the mycelium is growing which yields the conidia. I have found that if the attempt is made to infect a carrot with conidia taken directly from the *Botrytis* growing on artificial solutions it often fails, whereas the conidia produced on the carrot as a substratum succeed more easily; moreover, there was so much variability in the infections, and especially in the rate of progress of

* N.B.—This is one of the plants which is particularly rich in organic acids, and shows well the influence of warmth and daylight in diminishing them (see Warburg, *loc. cit.*, especially pp. 125, 132, 133, 134).

† 'Bot. Zeitg.,' 1886, col. 396 (see also the remark under *Sclerotinia Fuckeliana* in 'Comp. Morph. and Biol. of Fungi,' p. 380).

‡ A circumstance distinctly calculated to retard the decomposition of the acids and to bring about a tender etiolated condition.

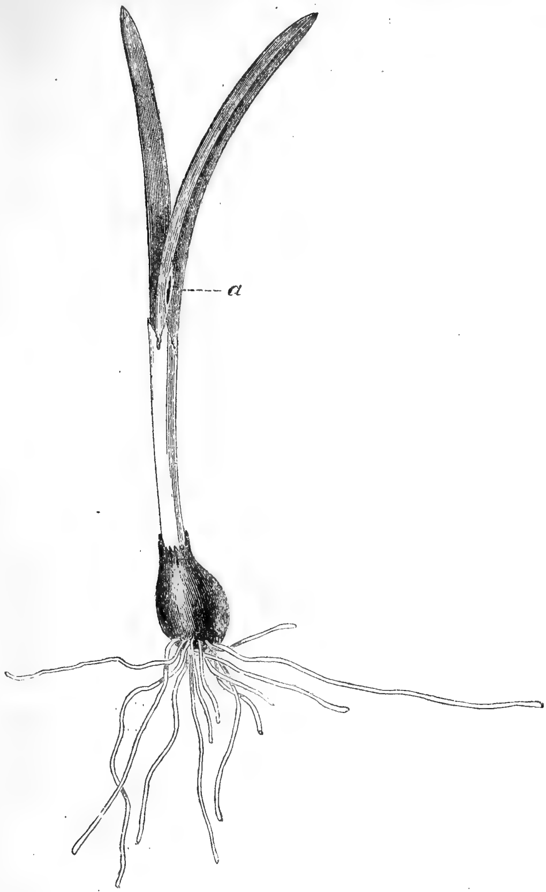


FIG. 11. A young snowdrop plant, artificially infected with *Botrytis*. The infection-spot (*a*) is sunken in the centre, and deep sienna brown or nearly black; the paler area around is yellowish-brown. The fungus hyphæ extend from this centre into the tissues, or not, according to circumstances. (See text.)

the infecting mycelium, that I was continually puzzled to account for the phenomena, and suspected that the conidia varied in infecting power, according to their size as well as the manner of culture. This would be a natural conclusion from what was already proved with regard to the invigoration of the mycelium, for, after all, conidia are only slightly specialised bits of mycelium cut off for purposes of rapid propagation,* and we may expect them to be directly affected by

* See Sachs, 'Lectures on Phys. of Plants,' p. 722; also de Bary's critical remarks in 'Comp. Morph. and Biol. of Fungi,' p. 129.

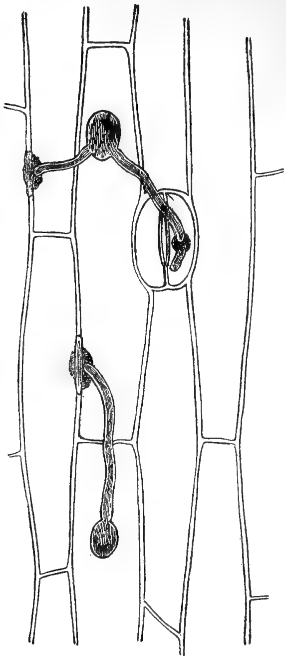


Fig. 12. Spores of *Botrytis* germinating on the epidermis of a snowdrop, and infecting it by means of their germinal tubes, the tips of which penetrate the cell-walls by means of secreted zymases, and cause them to turn brown at the points of entrance, as shown by the shading. (Highly magnified.)

everything which promotes or retards the vigour of the mycelium. I regard the conidium as distinct from any vegetative piece of mycelium chiefly in its capacity to form the necessary (cellulose-dissolving, &c.) ferment or zymase* in greater quantity or in a shorter time (with respect to the size of the organ), and look upon its size, shape, and colour, &c., as so many adaptations to the mode of life of the fungus.

Be this as it may, the conidia vary in infective power according to their nutrition—*i.e.*, according to the substratum on which the mycelium grows—and according to the generation to which the conidium belongs, *i.e.* (as I interpret it), according to the increasing vigour of the successive mycelia which produce the conidia.†

This latter fact is best demonstrated as follows: A crop of conidia is grown on a given pabulum, *e.g.*, on the moist sclerotium; the conidia are sown on the cut surface of ripe, sweet pears, and produce mycelia,

* See 'Annals of Botany,' vol. 2, 1888, p. 356.

† Kissling (*op. cit.*, p. 31) has proved this for *Botrytis cinerea*.

whence conidia again arise; these are again sown on pears, and produce a still more vigorous mycelium and crop of conidia, and so on. Calling the first crop of conidia generation I, and the second crop generation II, the third generation III, and so on, it was found that if the conidia of generation I are sown on similar leaves of a *Sempervivum* in a tiny drop of sap they do not infect the leaf; whereas those of generation II, similarly sown, infect the leaf at once, and those of generation III are still more virulent.

Kissling,* who has paid special attention to this point, and has carried the matter much further than other observers, measures the infective power of the conidia by the size of the disease-spot they produce in a given time.

As I have shown, the penetrating germinal hypha causes the cells in its neighbourhood to collapse and turn brown, because the excreted ferment or poison destroys the cellulose, and makes the protoplasm unable to retain the sap, which consequently suffuses and browns the area concerned. Now it is obvious that the rapidity with which this browning occurs may be taken as a rough measure of the progress —and, therefore, of the destroying power—of the infecting hyphæ, other things being equal. Well, Kissling took the necessary precautions, and set the conidia of succeeding generations I, II, and III to work on the surface of various fruits and other parts of plants, of course using the same substratum in any one series of experiments.

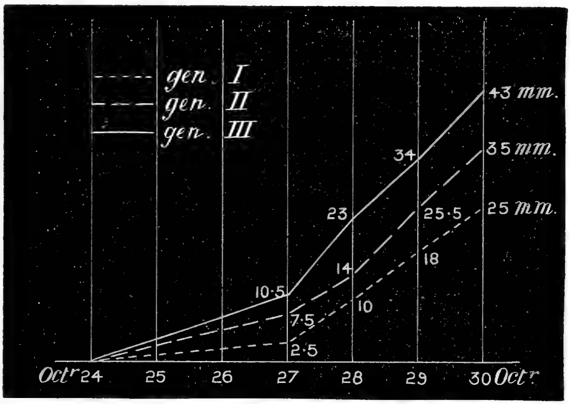


Fig. 13. Diagram constructed to show the relative progress of infections produced by successive generations of conidia of *Botrytis cinerea* (see text). The three different generations are denoted by differences in the characters of the curves. The horizontal base line is divided into six equal parts representing days; the distances measured on the ordinates represent the diameters of the disease-spots in millimetres, according to Kissling's experiments.

* *Loc. cit.*, pp. 22—29.

In a day or two the infected circular areas, as marked by the brown colour, were large enough to measure in millimetres, and by measurements on successive days he was able to judge of the progress of this extraordinary race, and he comes to the conclusion that on the same substratum the conidia vary, according to their generation, in their power of destroying the tissues. Those of the third generation, for instance, not only germinate more rapidly and vigorously—indications that they start in the race better equipped in the matter of food-materials and ferment-yielding substances—but they also destroy the cells of the host which they compete with more rapidly—which, no doubt, indicates that they are able to produce or manufacture more poison in the same time.

Summary of the Factors of an Epidemic—Bearing of the Discussion on other Parasitic Diseases—Conclusion.

It will be clear from the foregoing that in the case of an epidemic fungus disease, such as we have been considering, there are several classes of factors to be regarded, and I may sum up the chief points somewhat as follows. First, we have the normal healthy host-plant, with all its hereditary (internal) and adaptive peculiarities; secondly, we have the parasitic fungus, also with its disposition. Then we find, thirdly, that, apart from its inherent powers of variation, the host is subject to variable external influences during its life, which may produce such changes in the cell-walls and contents, &c., that the plant approaches nearer and nearer the limits of health, wide as we may regard these. On the other hand, we have, as a fourth consideration, the parasite also varying under the influence of changes in the factors of the environment, and its variations may, of course, be also dangerous to its welfare; but they may, on the contrary, be in such directions that it is enabled to profit by the counter-variations of the host. When the combined effects of the physical environment are unfavourable to the host, but not so or are even favourable to the parasite, we find the disease assuming a more or less pronounced epidemic character.

It is not pretended that we have here a totally new idea, because it has long been known that some organisms which bring about parasitic diseases do vary in the intensity of their effects, and can be made to do so artificially, and we know that some of the most brilliant results in biology have been obtained in connexion with certain lower organisms; but I have simply sought to show some of the links in the chain of causes and effects in the definite case of certain epidemic diseases of plants produced by the parasitism of some of the more highly developed fungi, and this, I think, has not been done before. If the preceding argument is admissible, new light will be thrown

not only on the cases of parasitism referred to, but also on the behaviour of the host in its struggle for existence with the factors of the inorganic environment, generally.

The question as to how far this view of the matter may be extended to other parasitic diseases of plants cannot be answered at present. Obviously the reflections excited will suggest lines of enquiry, and I may appropriately bring these remarks to a conclusion by a few brief comments on what is known as to the behaviour of other classes of parasitic fungi in this connexion.

Omitting the *Schizomycetes*, partly because they have a literature to themselves, and partly because they rarely* occur as parasites in the cells of plants, possibly owing to the acidity of the sap; and the *Myxomycetes*, of which Woronin's *Plasmodiophora*† is the best and most curious case; we have pronounced parasites (capable of producing epidemics) among the *Peronosporeæ*, one of which (*Phytophthora infestans*) has been more studied, probably, than any other true fungus parasite, at any rate so far as its life-history is concerned.‡

Much that has been stated in this lecture would, apparently, apply to the potato disease, and in view of the extreme interest that necessarily attaches to that malady, I draw attention to the following points of interest.

Suppose we take a potato plant the leaves of which are very slightly marked with minute disease spots, and divide it into two halves as exactly alike as possible, and place each half in a tumbler of water; the two tumblers, with their half-plants, are then placed in an ordinary room, side by side, at a temperature of about 20° C., and one is covered close with a bell-jar and the other left uncovered. In a short time—often a few hours—the covered leaves become black and rotten with the disease, whereas the uncovered one will go on looking fresh for several days, though it also succumbs at once if covered.§

The question arises whether the rapid spread of the fungus and the rot it causes here are simply owing to the increased supply of water, as the tissues become turgid in the saturated atmosphere under the bell-jar; or whether we have not here again, in addition, a case where the diminished access of oxygen to the interior of the tissues of the host results in an accumulation of organic acids and other substances which make the excessively turgid cells and thin, watery cell-walls more than usually easy prey to the parasite.

* Exceptions probably occur in the case of Wakker's hyacinth rot, the American "pear-blight," the "peach yellows," and a few others. See de Bary's 'Lectures on Bacteria,' 1887, p. 177, and the 'Reports of the U.S. Department of Agriculture,' especially No. 9, 1888.

† Pringsheim's 'Jahrb. f. wiss. Bot.' (1878, vol. 11, p. 548).

‡ See de Bary 'Morph. and Biology of the Fungi' for the chief literature.

§ See de Bary 'Die gegenwärtig herrschende Kartoffelkrankheit,' 1861, p. 55.

I ought to add, that if a potato plant is grown in a pot and kept under a bell-jar (untouched by *Phytophthora*) normally lighted, in the summer, the excessively watery dark-green shoots often develop hump-like outgrowths, composed of very large, thin-walled cells, which may be regarded as due to the excessive turgescence and hypertrophy of these cells. Presumably they contain relatively large quantities of organic acids, &c., and everything indicates that such a shoot would easily succumb to the *Phytophthora*, as in fact it does.

Kühn long ago noticed* that there are two periods when the potato shoot is most easily infected by the *Phytophthora*. The first is while still young—fully developed internodes are much more difficult to infect than young growing ones, a fact well known and easy to confirm; the second period is said by Kühn to occur after the tissues are far advanced, at the end of July or the beginning of August, and this would seem to be borne out by the experience of cultivators generally. I am inclined to regard this second period as coincident with the time when the plant is particularly rich in the products of assimilation on their way down to the tubers. They travel chiefly as glucose, and one consequence of the abundance of this carbohydrate, and the increased metabolism it supports, is an increase in the organic acids. If wet and dull weather sets in when the tissues are thus, so to speak, overflowing with such substances, the *Phytophthora* is peculiarly favoured, and can spread through the plant with the rapidity characteristic of an epidemic. In the allied genus *Pythium*, the phenomena are so similar that we may assume that the fungus behaves like Peronospora: the species are often saprophytes, however.

The question now arises, can these ideas be extended to the case of other parasitic fungi? It would be difficult to say with regard to the *Saprolegnieæ* and the *Mucorini*, because so little is known of their parasitism. As regards the *Ascomycetes* generally, we may expect great differences in respect to types like the *Gymnoasceæ*, *Rhytisma*, *Hysterium*, the *Erysipheæ* and the *Sphærias*, and they certainly occur.

Some *Nectrias* at any rate (†*e.g.*, *N. cucurbitula*, *N. cinnabarina*, and *N. ditissima*) behave so differently towards the host that we may probably conclude that the mode of procedure is unaffected by such variations on the part of the latter as have been sketched; and the same may be said of the wood-destroying *Hymenomycetes* (*e.g.*, *Agaricus melleus*, *Trametes radiciperda*, *Polyporus sulphureus*, &c.).

In all these cases the tree, as a whole, suffers in an indirect manner: these various cankers and rots, &c., destroy, for the most

* 'Ber. aus dem Physiol. Lab. u. d. Versuchsanstalt des Landw. Instituts d. Univ. Halle,' 1872, pp. 81—82, quoted by Sorauer, vol. 1, p. 140.

† 'Unters. a. d. Forstbot. Inst. zu München,' vol. 1, pp. 88 and 109, and vol. 3 also R. Göthe (Thiel's 'Landw. Jahrb.,' 1880, vol. 9, p. 837).

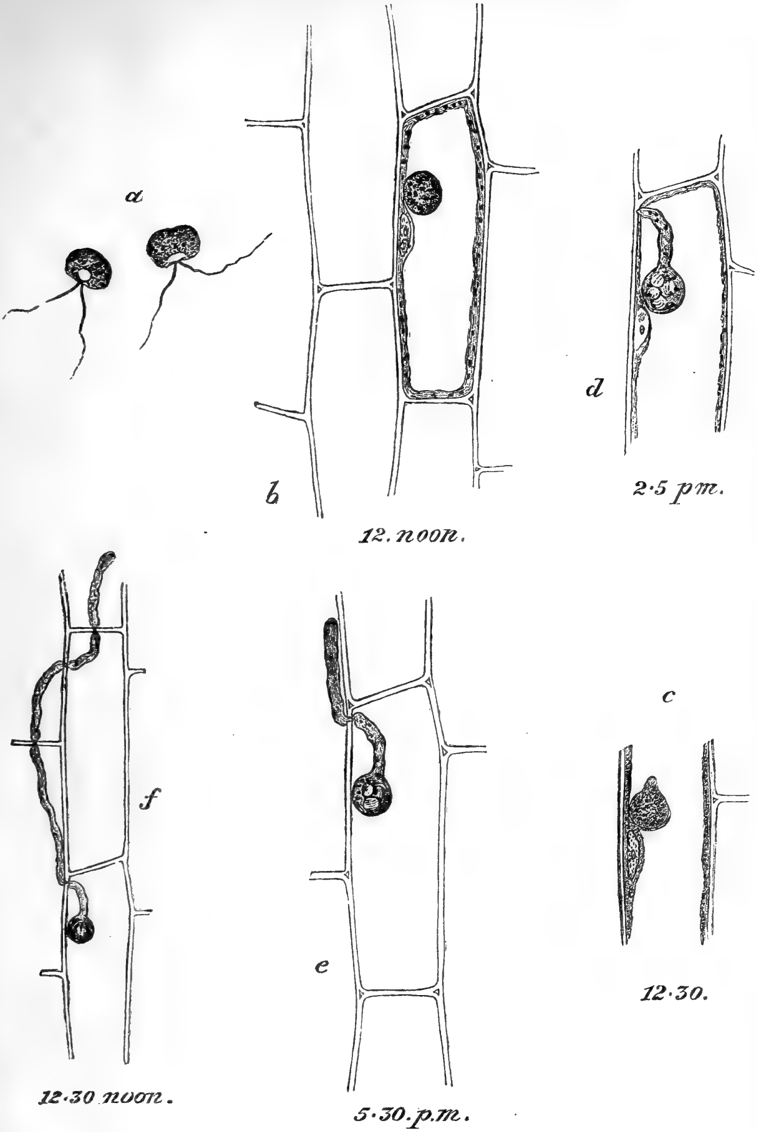


FIG. 14. Zoospores of a species of *Pythium* allowed to germinate in water on a piece of a longitudinal section of a bean-stem. The zoospores (*a*) soon come to rest, and one was noticed at mid-day on an exposed cell (*b*), lying nearly over its nucleus. At 12.30 this spore had begun to germinate and enter the cell (*c*). At 2.5 p.m. the germinal hypha had turned to the left, and commenced to bore through the side-wall with its tip (*d*). At *e* is shown the progress by 5.30 p.m. on the same day; and at *f* (smaller scale) the condition of affairs at 12.30 next day. That these hyphæ pierce the walls by means of secreted zymases can scarcely be doubted after what has been proved for *Botrytis*.

part, structures which are already dead, and so interfere with the transpiration current, and other large groups of functions, more by the mechanical injury done than by direct injury to living cells.*

In the group of the *Ustilagineæ* we have some of the most remarkable parasites known, and the relation of the host-plants (chiefly species of Gramineæ and Cyperaceæ) to them must be very different in detail.

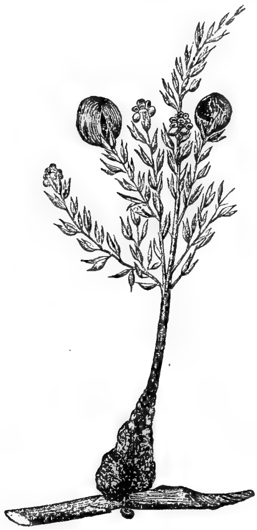


FIG. 15. *Zea mais.* Portion of inflorescence (reduced) with malformations produced by the parasitism of the fungus *Ustilago Maidis* (Sorauer).

In the more typical cases we find that the sporidia or conidia germinate in artificial nutritive media, and go on producing generation after generation of their like,† and this undoubtedly occurs in the open fields, &c. Brefeld states that he has cultivated one form through more than a hundred successive cultures in the course of a whole year, and that this corresponds to about 1500 successive sprout-series or generations,‡ but towards the end of the period the *germinating* power of the successive conidia became weaker and weaker, and at last failed.

* The germinal hyphæ developed from the spores of such fungi often find their way into the wood, cambium, &c., by means of wounds, caused by mechanical breakage, the nibbling of mice, squirrels, the punctures of insects, frost-cracks, the blows of hailstones, and so forth, which introduce us to a different aspect of the relations between host and parasite.

† Brefeld, 'Bot. Unters. über Hefenpilze,' part 5, Leipzig, 1883.

‡ Brefeld, in a lecture before the *Klub der Landwirthe zu Berlin*, 1888 ('Nachrichten aus d. Klub d. Landw. zu Berlin,' 1888, Nos. 220—222).

He also found that the conidia germinate, by developing a germinal tube only at a given period, and not at any indefinite time they may happen to be sown: consequently they are unable to infect the host unless they happen to be on the proper spot at the right time.

Now Kühn showed long ago* that the infection of the cereal by *Ustilago* can only occur near the "collar" of the young germinating seedling, and although differences have arisen† as to the exact spot in this region which alone can be infected, there is one point on which all are agreed, namely, that the germinal tube can only enter the young actively growing tissues in that region. Immediately the first internode is completed, the seedling is proof against infection in the open.‡

That it is really a matter of the age and condition of the tissue is beautifully demonstrated by Brefeld, who showed that if the conidia are forced into the bud by means of a syringe, so that they can germinate in contact with the embryonic cells at the growing point, infection may be ensured at any time.

But far the most interesting point about these fungi is that when the germinal hypha is once inside the tissues it goes on growing with them, keeping between the cells. Although we have almost no information as to details here, it can hardly be doubted that the chief agent in maintaining the balance of position in this case is the living substance in the cells of the host; but I know of no explanation for this beyond the general one, that so long as the cells of the internodes are actively performing their normal functions, the hyphæ have to be contented, so to speak, with a sort of suppressed existence in the intercellular spaces and middle lamella of the walls.

True, when the young fruits begin to fill out, the mycelium accomplishes a sharp revenge by destroying the whole fruit; but it is obvious that the relations which determine the epidemic or sporadic character of the disease are those between the tissues and the germinal hyphæ and young mycelium, and there are great variations in these matters, even in the group of the *Ustilagineæ* itself.§

Very different again must be the relations in detail between host and parasite in the case of those *Uredineæ* which cause epidemic leaf diseases, especially those which form haustoria,‖ and it is almost impossible

* 'Krankh. d. Kulturpflanzen,' 1859, p. 46.

† *Cf.* Wolff, 'Brand des Getreides,' Halle, 1874; Hoffmann, 'Bot. Unters.,' 1866; 'Ueber den Flugbrand,' p. 206; Kühn, "Beobachtungen ü. d. Steinbrand d. Weizens" ('Oesterr. Landw. Wochenschr.,' 1880).

‡ This no doubt explains the fact that in a wet spring nearly all seeds with spores attached become infected, because the tissues remain in a youthful condition longer than in a dry season.

§ *E.g.*, contrast the behaviour of *Protomyces*, *Entyloma*, and *Ustilago Maidis*, for instance, with that of most other *Ustilagineæ*.

‖ In all cases where haustoria are developed the mycelium enters into a peculiar

to say anything about their nutrition beyond the general statement that they must have established such close temporary relations with the living cells of the host that their protoplasm and that of the host can go on absorbing nutriment from the same sources. This would seem to be proved by the curious phenomena of hypertrophy which they induce, *e.g.*, the young shoots of *Euphorbia cyparissias* are entirely altered in habit by the *Æcidium* of *Uromyces Pisi*, and the well-known "witches' brooms" of the silver fir,* for although the changes induced

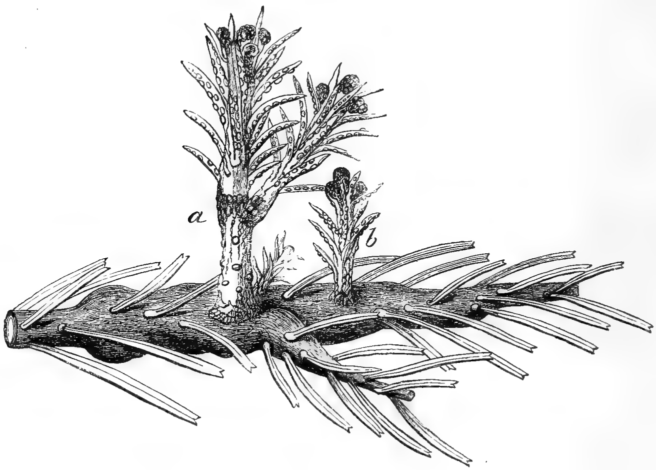


FIG. 16. A specimen of "witches' broom," on the silver fir, caused by the stimulating action of the Uredinous fungus *Æcidium elatinum*, the mycelium of which lives perennially in the cortex, &c., of the fir, and causes some of its buds to grow up into erect shoots of totally different habit from the normal branches. The blister-like Æcidia are visible on the leaves at *a* and *b* (Hartig).

imply that the cells are carrying out their functions in a modified manner, still they grow, divide, and evidently discharge their main duties much as usual. Consequently it is impossible to believe that any individual cell suffers much direct injury, and at least the protoplasm and nucleus, and even the chlorophyll corpuscles, &c., may re-

symbiotic connexion with the cells, and for some time merely taxes them, as it were, rather than injures them directly. Of course, there is ultimate injury, and even death, brought about in these cases; but how much this differs in different cases is evident from comparison of other fungi, like *Peronospora parasitica*, *Podosphæra Castagnei*, with Uredines such as *Hemileia vastatrix*, *Melampsora Goeppertiana*, &c.

* Such hypertrophies are not confined to the Uredineæ, however: *cf.*, de Bary, 'Morph. and Biol. of Fungi,' 368—369, and Zopf (Schenk, 'Handb.,' vol. 4, 1889 pp. 504—507).

main intact for weeks or months. No doubt in these cases also the entrance of the hyphæ or haustoria into the tissues is aided by any factors which cause the cell-walls to be softer or thinner than in the normal condition; and it is certain that many failures by those who have experimented with Uredinous fungi are attributable to their sowing the spores on older, well matured tissues.

We are here, however, abandoning the subject of the present lecture, because, in the first place, the phenomena just referred to appertain to sporadic rather than epidemic diseases, and because, secondly, they tend to the subject of *symbiosis* proper, where the relations between the host and the parasite have become so arranged that both may be said to benefit by the commensalism, as exemplified in the lichens, and some of the recently described cases of mutualism between fungi and the roots of Phanerogams.

April 17, 1890.

Sir G. GABRIEL STOKES, Bart., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Preliminary Note on Supplementary Magnetic Surveys of Special Districts in the British Isles." By A. W. RÜCKER, M.A., F.R.S., and T. E. THORPE, Ph.D., B.Sc. (Vict.), F.R.S. Received March 5, 1890.

During the summer of 1889 we carried out additional magnetic surveys of the Western Isles and the West Coast of Scotland, and of a tract of country in Yorkshire and Lincolnshire.

Both districts were selected with special objects in view. We had found that powerful horizontal disturbing forces acted westwards from the Sound of Islay, from Iona, and from Tiree, and we had deduced a similar direction for the disturbing force at Glenmorven from Mr. Welsh's survey of Scotland in 1857–58. The whole district presents peculiar difficulties, partly from the fact that local disturbance is likely to mask the effects of the regional forces, partly because the normal values of the elements must be especially uncertain at stations on the edge of the area of our survey.

If, then, the general westward tendency of the horizontal disturb-

ing forces was due to some source of error, stations in the extreme south of the Hebrides would in all probability be similarly affected. If the directions of the forces were due to a physical cause, such as a centre of attraction out at sea to the west of Tiree, then the disturbing forces in the Southern Hebrides would almost certainly be directed southwards towards it.

The observations made last summer prove (1) that the direction of the disturbing horizontal force at Bernera, which is the southernmost island of the Hebridean group, is due south; and (2) that, as this point is approached from the north, the downward vertical disturbing attraction on the north pole of the needle regularly increases, which exactly agrees with the supposition that a centre of attraction is being approached.

There is, therefore, now no doubt that there is a centre of attraction on the north pole of the needle to the south of the Hebrides and to the west of Tiree.

(3) In one of the maps communicated to the Society last year we drew two lines, bounding a district about 150 miles long and 40 miles broad, in Yorkshire and Lincolnshire, and gave reasons for the belief that a ridge line or locus of attraction lay between them.

This conclusion has now been tested by means of thirty-five additional stations, with the following results :—(1) At all stations (with one exception) on or near the two lines, the horizontal disturbing forces tend towards the centre of the district they bound.

(2) The downward vertical disturbing forces are greater in the centre of the district than at its boundaries. In particular, there are two well-marked regions of very high vertical force.

(3) The greatest vertical force disturbances occur at Market Weighton, where the older sedimentary rocks are known to approach the surface, and at Harrogate, which is on the apex of an anticlinal.

(4) The central ridge line runs from the Wash parallel to the line of the Wolds to Brigg. Thence it appears to turn west, and reaches Market Weighton *viâ* Butterwick and Howden. One or two additional stations are, however, required to determine whether this bend is real, or whether the line runs direct from Brigg to Market Weighton. From the latter town it passes to the limestone district of Yorkshire and traverses its centre. It has not yet been traced west of the line of the Midland Railway between Settle and Hawes, but there is ground for believing that it continues to the Lake District.

Although, therefore, one or two points of detail remain for further investigation, the existence of a line of attraction 150 miles long is proved beyond the possibility of doubt, and for about 90 miles its position is known to within 5 miles.

There are, then, even in those parts of England where the super-

ficial strata are not magnetic, regions of high vertical force comparable in size with small counties, and ridge lines or loci of attraction as long and almost as clearly defined as the rivers. Their course is closely connected with the geology of the districts through which they run.

II. "The Variations occurring in certain Decapod Crustacea.—I. *Crangon vulgaris*." By W. F. R. Weldon, M.A., Fellow of St. John's College, Cambridge, and Lecturer on Invertebrate Morphology in the University. Communicated by Professor M. Foster, Sec. R.S. Received March 20, 1890.

It is well known that two sets of animals, belonging to the same species, but living in different places, exhibit differences from one another by which they can, in many cases, be easily distinguished. But it is at the same time equally certain that the forces determining the differences between local races of the same species do not so act as to produce the same effect upon all individuals of the same race: for I am aware of no case in which the individuals composing any race of animals—however small and isolated the area in which they live, however uniform the conditions which obtain throughout that area—have been shown to resemble one another *exactly* in any character.

Since the adjustment of a local race to the average proper to it is not complete, the question arises, whether it is not possible to determine the degree of accuracy with which this adjustment is effected, and the law which governs the occurrence of deviations from the average. The object of this paper is to give an account of certain observations made at the laboratory of the Marine Biological Association at Plymouth, in order to determine, *first*, the average length of three or four organs which admitted of accurate measurement, and, *secondly*, the frequency with which the average length and every deviation from it occurred in one or two local races of *Crangon vulgaris*.

In making this investigation, I have had the great privilege of being constantly advised and helped, in every possible way, by Mr. Galton. My ignorance of statistical methods was so great that, without Mr. Galton's constant help, given by letter at the expenditure of a very great amount of time and trouble, this paper would never have been written. I am glad to take this opportunity of expressing my gratitude for his generous conduct. I have also to thank Dr. Donald MacAlister for explaining to me many points connected with the law of error, and for helping me in various ways.

Mr. Galton has, as is well known, studied the frequency with which deviations from the average size of certain organs occur in man, in certain plants, and in moths. The result of his investigations has been to show that deviations from the average size of the organs measured by him occur in every case with the frequency indicated by the law of error. These results were, however, based on an investigation either of civilised man, or of a domesticated animal or plant: and Mr. Galton has himself pointed out that in the majority of cases studied, the effect of natural selection is probably insignificant. The similar investigations of Quetelet and others are also confined to civilised man. It has, therefore, seemed worth while to attempt an investigation of the variations in the size of certain organs which occur in a species living in a wild state, upon which natural selection and the other destructive or plastic influences from which domestic animals and civilised man are alike protected may be supposed to act with full effect.

In his recent work on heredity,* Mr. Galton predicted that selection would not have the effect of altering the law which expresses the frequency of occurrence of deviations from the average: so that he expected the frequency, with which deviations from the average size of an organ occurred, to obey the law of error in all cases, whether the animals observed were under the action of natural selection or not. The results of the observations here described are such as to fully justify Mr. Galton's prediction.

These observations relate entirely to the lengths of organs, or parts of organs. The measurements of these lengths were made either with a pair of compasses, in the case of the greater lengths, or in the case of smaller parts by means of a microscope, provided with cross-wires, and travelling on a screw of known pitch. The results are, I believe, accurate to within about 0·1 mm. The edges of the parts measured were in many cases so uneven, and the effect of the spirit in which the specimens were preserved was probably so considerable, that a greater degree of apparent accuracy in the measurements would not have implied a more reliable result.

In order to compare the organs of one individual with the corresponding parts of another individual of different size, it was evidently necessary to express the dimensions of each organ in terms of the length of the body of the individual to which it belonged. All the measurements used in this paper are therefore, expressed in terms of the total length of the body, which is taken as 1000.

Having obtained measures of the length of an organ in a sufficiently large sample of individuals, the frequency with which the various magnitudes occur may be conveniently exhibited in the fol-

* 'Natural Inheritance,' pp. 119—124.

lowing way, which is that adopted by Mr. Galton:—The values obtained are sorted and arranged in order of magnitude; then, at equal distances along a given base, ordinates are erected equal in number to the observations, one ordinate being proportional to each observed value of the organ. By joining the tops of these ordinates, a curve is obtained such as that drawn in fig. 2.

If the base-line of such a curve be divided into one hundred parts, then the proportion of individuals measured, which possess the organ from which the curve is constructed, of a size greater or less than any given magnitude, can be readily ascertained. For example, in fig. 2, which shows the distribution of lengths of the carapace in 400 female shrimps from Plymouth, the ordinate, whose length is 256, stands at grade 20°, showing that 20 per cent. of the individuals examined had the carapace longer than 256 (the body length being 1000), while in the remaining 80 per cent. the carapace was shorter than this.

A curve constructed in the manner directed is nearly always symmetrical about its middle point: and this point therefore closely approximates to the average of the whole number of observations from which the curve was constructed. The value of the middle ordinate will always be taken, in what follows, as the average value: it will, in accordance with Mr. Galton's notation, be spoken of as the Median, and denoted by the symbol M. Each curve, therefore, gives by simple inspection the average value of the organ to which it refers.

In estimating the *deviations from the average* which occur in each case, the magnitude of the average itself is evidently of no importance: and the ordinates of the curve may therefore be considered with reference to an axis passing through the point M, so that the ordinate of M becomes zero. When measured from this axis, half the ordinates of the curve are of course positive, the other half being negative.

If the frequency, with which the observed deviations from the average occur, obeys the law of error, then the curve just described should be a "curve of error," whose "probable error" is represented by the ordinates at the 25th and 75th grades. These grades are the boundaries of the first and third quarters of the base: they will, therefore, be spoken of (again in accordance with Mr. Galton's notation) as Quartiles, and will be denoted by the symbols Q_1 and Q_3 respectively. In a perfectly normal curve, Q_1 and Q_3 are of course equal in magnitude and opposite in sign. In the observed curves there was generally a slight difference between the two: and the mean value of the two is therefore adopted as the "probable error," which will be denoted by the symbol Q.

In order to determine the correspondence between the observed

curve and the curve of error, the ordinates of the observed curve will be compared with those of the curve of error at certain fixed grades.

This may be most conveniently done by considering the ordinates of the curve of error at those grades as multiples of the "probable error" of the curve.

The grades chosen, together with the ordinates of a curve of error, expressed in terms of its probable error, at those grades, are as follows:—

Table I.—Ordinates of a Curve of Error, in Terms of Q.

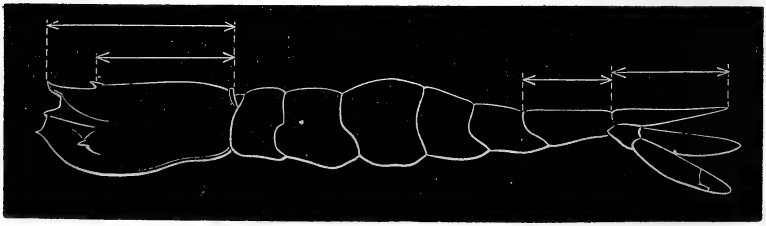
Grade.	Ordinate = Q ×	Grade.	Ordinate = Q ×
5°	+2·44	60°	−0·38
10	+1·90	70	−0·78
20	+1·25	75	−1·00
25	+1·00	80	−1·25
30	+0·78	90	−1·90
40	+0·38	95	−2·44
50	0·00		

It will be seen that, in order to compare a curve constructed from a number of observations with a curve of error, the following process is performed: the ordinates at the selected grades are determined, and the observed value of M is subtracted from each of these. The remainders, divided by $\frac{1}{2}(Q_1 - Q_3)$, should give the coefficients of Q which appear in the above table.

Such a comparison will now be made between the curve of error and the curves obtained from the measurements.

The organs measured are four: the total length of the carapace; the distance from the posterior margin of the carapace to the front of the median spine; the length of the sixth abdominal tergum; and the length of the telson. The parts are shown in the accompanying woodcut.

Fig. 1.

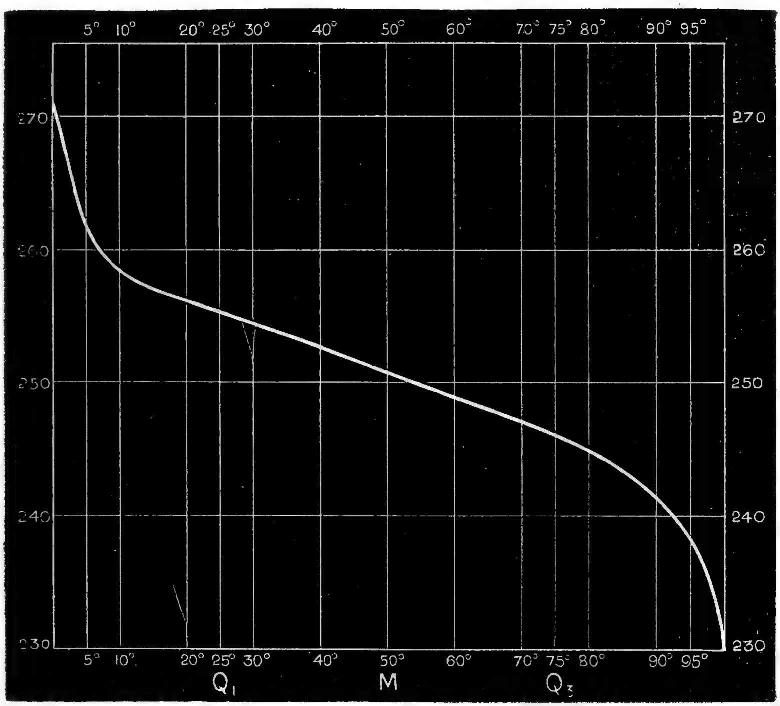


In measuring the length of the sixth tergum and of the telson, these organs were removed from the body; so that the small portion of each which projects inwards into a fold of skin, and serves as an attachment for muscles, is included in the total length.

The individuals measured were all adult females; they were collected from widely separate places. The first sample measured consisted of 400 individuals from Plymouth Sound; a second sample, containing 300 females, was obtained from Southport by Mr. W. Garstang; and a third sample, of which 300 were measured, was sent to me from Sheerness by Mr. W. H. Shrubsole.

Total Length of the Carapace.—The curve obtained by treating the total length of the carapace of the Plymouth specimens in the way described is shown in the woodcut, Fig. 2.

FIG. 2.



It will be seen that the median ordinate has a length of 250·52, which is therefore the value of M. The ordinate at 25° is 255·07 = M+4·55; that at 75° is 246·00 = M−4·52; so that the mean value $Q = \frac{1}{2}(4{\cdot}55+4{\cdot}52) = 4{\cdot}53$.

The following table will show the relation between this curve and the normal curve of error:—

Table II.—Curve of Distribution of Carapace Lengths—Plymouth.

Grade.	Ordinate.	Ord. − M.	$\frac{\text{Ord.} - \text{M}}{\text{Q.}}$	Normal curve.
5°	261·50	+10·98	+2·42	+2·44
10	258·95	+ 8·43	+1·86	+1·90
20	256·05	+ 5·53	+1·22	+1·25
25	255·07	+ 4·55	+1·00	+1·00
30	254·10	+ 3·58	+0·79	+0·78
40	252·27	+ 1·75	+0·39	+0·38
50	250·52	0·00	0·00	0·00
60	249·09	− 1·43	−0·32	−0·38
70	247·29	− 3·23	−0·71	−0·78
75	246·00	− 4·52	−1·00	−1·00
80	244·74	− 5·78	−1·28	−1·25
90	241·39	− 9·13	−2·10	−1·90
95	238·60	−11·92	−2·63	−2·44

It will be seen therefore that the average length of the carapace in the Plymouth specimens was 250·52-thousandths of the body length, and that the frequency with which this length and the various observed deviations from it occurred was almost exactly that indicated by a curve of error whose prob. error = 4·53.

In the above table, all the steps in the determination of the coefficients of Q are indicated. To indicate all the steps in this determination in the case of each race would involve a great deal of vain repetition; and, therefore, in the following table the coefficients themselves are alone indicated. It will be understood that the entry opposite each grade in this table is found by subtracting the value of M from the observed ordinate at that grade, and dividing the remainder by $\frac{1}{2}(Q_1-Q_3)$. The value thus obtained should be the coefficient of Q in the table of ordinates of a normal curve given on page 448. These coefficients are repeated in the last column of the following table.

Table III.—Ordinates of the Curves of Deviation of Carapace Lengths, each in Terms of its own Q.

$$\text{Each entry} = \frac{\text{observed ordinate} - \text{M}}{\text{Q}}.$$

Grade.	Plymouth. 400 specimens.	Southport. 300 specimens.	Sheerness. 300 specimens.	Normal curve.
5°	+2·42	+2·86	+3·34	+2·44
10	+1·86	+2·11	+2·29	+1·90
20	+1·22	+1·29	+1·35	+1·25
25	+1·00	+0·97	+1·02	+1·00
30	+0·79	+0·70	+0·76	+0·78
40	+0·39	+0·34	+0·35	+0·38
50	0·00	0·00	0·00	0·00
60	−0·32	−0·37	−0·35	−0·38
70	−0·71	−0·80	−0·74	−0·78
75	−1·00	−1·03	−0·99	−1·00
80	−1·28	−1·27	−1·28	−1·25
90	−2·10	−2·05	−1·97	−1·90
95	−2·63	−2·68	−2·41	−2·44

The table shows that in all the races the coefficients of Q agree fairly well with those indicated by the normal curve. When these coefficients and the values of M and Q in each case are known, it is evident therefore that the whole curve is known.

The values of M and Q are as follows:—

Plymouth.......... M = 250·05; Q = 4·53
Southport .... M = 248·50; Q = 3·17
Sheerness.......... M = 247·51; Q = 3·05

It thus appears that not only does the average size of the carapace differ in different local varieties, but the range of deviation from that average differs also. Nevertheless, the frequency with which the observed deviations from the average occur is in all the three observed cases expressed by a curve of error.

Since the variations observed in adult individuals depend not only on the variability of the individuals themselves (which is possibly nearly alike in all races), but also upon the selective action of the surrounding conditions—an action which must vary in intensity in different places—the result here obtained is precisely that which might be anticipated, and it is precisely that predicted by Mr. Galton.

The same features are presented by the curves derived from the remaining sets of measurements. The following tables give the data for constructing curves of deviation of each organ.

Table IV.

Grade.	Post-spinous portion of carapace.			Length tergum VI.*		Length telson.*		Normal curve.
	Plymouth. M = 178·03. Q = 3·58.	Southport. M = 179·50. Q = 3·01.	Sheerness. M = 179·63. Q = 2·80.	Plymouth. M = 145·46. Q = 3·24.	Southport. M = 141·72. Q = 3·02.	Plymouth. M = 192·44. Q = 4·56.	Southport. M = 195·45. Q = 3·52.	
5°	+2·35	+2·76	+2·95	+2·60	+2·32	+2·40	+2·95	+2·44
10	+1·79	+2·01	+2·27	+2·04	+1·87	+1·78	+2·27	+1·90
20	+1·23	+1·16	+1·37	+1·24	+1·12	+1·22	+1·37	+1·25
25	+0·98	+0·97	+1·08	+0·95	+0·88	+0·97	+1·08	+1·00
30	+0·75	+0·76	+0·85	+0·66	+0·69	+0·73	+0·85	+0·78
40	+0·34	+0·39	+0·41	+0·31	+0·32	+0·35	+0·41	+0·38
50	0·00	0·00	0·00	0·00	0·00	0·00	0·00	0·00
60	−0·29	−0·37	−0·37	−0·35	−0·39	−0·35	−0·37	−0·38
70	−0·75	−0·79	−0·73	−0·78	−0·86	−0·80	−0·73	−0·78
75	−1·02	−1·03	−0·91	−1·05	−1·12	−1·02	−0·91	−1·00
80	−1·35	−1·50	−1·18	−1·30	−1·35	−1·23	−1·18	−1·25
90	−2·11	−1·80	−1·88	−1·84	−1·85	−1·89	−1·88	−1·90
95	−2·58	−2·30	−2·48	−2·38	−2·30	−2·39	−2·48	−2·44

* The lengths of tergum VI and of the telson were not determined in the Sheerness specimens.

That the deviations from the normal value of the coefficients of Q shown in the foregoing are accidents due to the small number of observations upon which the curves are based is shown by the following table, in which each entry is the mean of the corresponding entries of all the preceding tables :—

Grade.	Mean of observed coefficients of Q.	Normal.	Grade.	Mean of observed coefficients of Q.	Normal.
5°	+2·55	+2·44	60°	−0·36	−0·38
10	+1·99	+1·90	70	−0·78	−0·78
20	+1·24	+1·25	75	−1·03	−1·00
25	+0·97	+1·00	80	−1·31	−1·25
30	+0·74	+0·78	90	−1·97	−1·90
40	+0·35	+0·38	95	−2·49	−2·44
50	0·00	0·00			

Results similar to the above have been obtained from measurements of a larger series of organs, and parts of organs, in *Pandalus annulicornis* (two races) and *Palæmon serratus* (one race); but, at present, not more than 100 individuals of each race have been measured, and the curves of distribution of the magnitudes of the various organs are therefore more irregular than those given for the shrimp. In these cases, however, there is no constant deviation in any direction from the normal curve. There seems, therefore, no reason to doubt that an extended series of measurements will show that the variations of these animals obey the law of error as closely as do those of *Crangon*. I hope shortly to collect such a series of measurements.

It seems, therefore, that Mr. Galton's prediction is fully justified; and that (1) the variations in size of the organs measured occur with the frequency indicated by the law of error; and (2) the "probable error" of the same organ is different in different races of the same species.

I have attempted to apply to the organs measured the test of *correlation* given by Mr. Galton ('Roy. Soc. Proc.,' vol. 45, No. 274, pp. 135 *et seq.*); and the result seems to show that the degree of correlation between two organs is constant in all the races examined. Mr. Galton has, in a letter to myself, predicted this result. A result of this kind is, however, so important to the general theory of heredity, that I prefer to postpone a discussion of it until a larger body of evidence has been collected.

III. "Observations on the Anatomy and Development of *Apteryx*." By T. JEFFERY PARKER, B.Sc., F.R.S., Professor of Biology in the University of Otago, Dunedin, New Zealand. Received March 20, 1890.

(Abstract.)

The chief materials for the present investigation consist of a number of embryos of the three common species of *Apteryx*, which naturally group themselves into ten stages (A—K); an eleventh stage (L) is furnished by a bird a few weeks old, a twelfth (M) by the skeleton of an adolescent specimen, and a thirteenth (N) and fourteenth (O) by odd bones of young birds; the adult may be considered as constituting a fifteenth stage.

The embryos were, for the most part, well preserved, but not sufficiently well for the purposes of exact histological study. The single embryo belonging to stage A corresponds in most respects to a chick of the fourth day.

The author returns his sincere thanks to the Council of the Royal Society for the grant from which the expenses of the investigation were defrayed, and also to those who have assisted him in various ways. His paper is illustrated with seventeen plates, depicting the external form and anatomy of the various stages, and a number of new terms are proposed in the description of the skeleton.

The following account is abstracted from the author's summary of results:—

External Characters.—In stage C, corresponding with a sixth-day chick, there is a well-marked operculum growing backwards from the hyoidean fold, and covering the third (? and fourth) visceral cleft. A rudiment of this structure is seen in the preceding stage.

In stage A, the limbs have already attained their permanent position, so that, if the backward shifting of the appendages so noticeable in the chick occurs in *Apteryx*, it must take place at an unusually early period.

From the first appearance of the feather papillæ there are well-marked pterylæ and apteria, most of which can be made out with tolerable distinctness in the adult.

The wing of the adult has a well-marked pre- and post-patagium, and amongst its feathers may be distinguished nine or ten cubitals, two or three metacarpals, one mid-digital, and a row of tectrices majores. The barbicels of the feathers are slightly curved.

The fore-limb passes through a stage in which it is a tridactyle paw with subequal digits, followed by one in which it is a typical

wing with hypertrophied second and partially atrophied first and third digits.

The nostril has acquired its final position at the end of the beak in stage E; up to the middle of incubation the whole respiratory region of the olfactory chamber, from the anterior nares to the commencement of the turbinals, is filled with a solid mass of epithelial cells, through which a passage is formed at a later period.

At no stage is there any trace of the caruncle or "egg-breaker" at the end of the beak.

The Law of Growth.—A number of details are given with respect to the various proportions of the different parts at different ages.

The Specific and Sexual differences observable in the three species are described.

The Skull.—In stages A and B the only cranial rudiments present are the parachordal plates, continued cephalad into the prochordal plate, and the visceral arches.

In stage C the trabeculæ have appeared, and are continuous with the parachordals; the prochordal plate sends off paired processes directly upwards in the mesencephalic flexure and laterad of the third nerves.

In stages E and F the pituitary fossa is pierced by three apertures in longitudinal series—the anterior, middle, and posterior basicranial fontanelles. The middle fontanelle has disappeared in stage G, but the anterior and posterior are still recognisable in stages H and I. Through the anterior fontanelle the pituitary radicle passes.

The medio-dorsal portion of the dorsum sellæ arises as a distinct chondrite, the prochordal cartilage, which in stages F and G is quite separate both from the trabecular and from the parachordal regions of the skull.

None of the stages show a separate prenasal cartilage or intertrabecular; if present as a distinct chondrite it certainly does not extend further backwards than the anterior presphenoidal region; the posterior presphenoidal region is clearly formed from the trabeculæ.

In stages D, E, and F the presphenoid is a vertical plate of considerable antero-posterior extent, and gives origin to a pair of large orbitosphenoids. In stage A the orbitosphenoids have begun to atrophy, and in later stages are reduced to narrow bars of cartilage, the presphenoid at the same time undergoing a great diminution in antero-posterior extent.

The olfactory capsules extend backwards to the optic foramina mesiad of the eyes; there is at no stage an interorbital septum.

The turbinals are unusually well developed, and are divisible into anterior, middle, posterior, anterior accessory, ventral accessory, and mesoturbinal folds. Alone amongst these, the anterior accessory turbinal is formed as a hollow invagination of the wall of the olfactory capsule,

not as a plate-like ingrowth; its cavity contains a prolongation of the antrum of Highmore.

There are paired, rod-like Jacobson's cartilages, lying one on each side of the rostrum in the vomerine region.

In late embryonic life, and even in the adult, the quadrate articulates with the roof of the tympanic cavity by a double articular surface.

The hyoidean portion of the tongue-bone chondrifies late—subsequently to stage G—and never ossifies.

The Vertebral Column.—As in other Birds, the atlas arises from a post-occipital intercentrum and a pair of neurochondrites. The axis consists originally of seven pieces. In both vertebræ each of these elements ossifies separately.

The way in which the notochord is constricted by the ingrowing centrochondrites differs greatly in the various regions.

The atlas and axis in a newly-hatched embryo differ far less than in the adult from those of the other Ratitæ.

Two intercentra are described in the caudal region.

A new method of writing the vertebral formula of birds is adopted.

*The Sternum and Ribs.**—The development of these parts seems to show that the costal sternum does not originate by the union of all four sternal ribs, but that it extends backwards independently of the third and fourth ribs, meeting them in turn and becoming united with them by joints.

In some adult specimens the sternum bears a low, median ridge, probably to be looked upon as a vestigial keel.

The form of the adult sternum is very variable.

The Shoulder Girdle.—Up to stage H the shoulder girdle is a single cartilage; during that stage the procoracoid and coracoid are differentiated by fenestration. The procoracoid degenerates into a ligament, which is sometimes present in the adult. The coracoid fenestra may persist or may be filled up by a preaxial extension of the coracoid.

Acromial, procoracoid, and acrocoracoid tuberosities are present.

The coraco-scapular angle varies from 150° to 122°. In stage E the scapula is curved backwards over the ribs. In the same stage the coraco-vertebral angle is 35°; by stage H it has increased to 90°.

The adult shoulder girdle is subject to great variation, both in form and size.

The Fore-limb.—In the carpus a radiale, an ulnare, and the three

* It is mentioned by the author that uncinate processes (or "uncinates") are present in the ribs of *Dinornis*, some points in the structure of the foot of which bird are also described.

preaxial distalia are distinguishable in early stages. The distalia usually concresce with the second and third metacarpals to form a carpo-metacarpus, with which the radiale and ulnare may or may not become united.

The pollex usually atrophies at an early stage, but a vestige of it may persist.

The manus is fairly constant in structure in *A. australis* and *A. Oweni*, but is very variable in *A. Bulleri*.

The Pelvic Girdle.—The pubis and ischium are nearly vertical in stages D and E, and gradually become rotated backwards.

The post-ilium is already fully formed in stage D, the pre-ilium not until stage G.

The pectineal process is ossified equally from the ilium and the pubis.

The Hind-limb.—In the tarsus a tibiale, a fibulare, and a single distale are distinguishable in stages D and E. In F a post-axial centrale appears in the rudiment of the mesotarsal articular pad; in G it becomes chondrified, and in the adult ossified. A smaller pre-axial centrale is first seen as a distinct chondrite in stage L; in the adult of *A. australis* and *A. Haasti* (?) it was observed as a separate bone in the preaxial moiety of the mesotarsal pad.

In stage D the fifth digit is represented by an elongated metatarsal; in E this has diminished in size, and in F undergone almost complete atrophy.

Muscles of the Wing.—The following muscles are present in the wing in addition to those described by Owen:—Brachialis anticus, supinator, pronator, anconeus, flexor profundus internus, extensor carpi ulnaris, extensor metacarpi radialis brevis, extensor indicis proprius, and flexor digitorum profundus. There may also be a brachialis anticus accessorius, an interosseus dorsalis, and probably a flexor carpi radialis.

The biceps arises from the acrocoracoid, the triceps by a long head from the scapula and by a short head from the humerus.

The Brain.—The mesencephal is unusually small from the first; in stages D—F the optic lobes are dorsal; in G they become lateral by the transverse extension of the optic commissure or median portion of the roof of the mesocœle; in H they are already ventral, although larger proportionally than in the adult.

The diencephal becomes tilted backwards in later stages, its dorsal wall becoming posterior and the foramen of Monro postero-dorsal instead of antero-dorsal.

The anterior commissure and corpus callosum are large.

The cerebral hemispheres are of unusual proportional length, and partly cover the cerebellum.

The Eye.—A pecten is present during late embryonic life.

Phylogeny.—The following characters support the view that *Apteryx* is derived from a typical avian form capable of flight:—

(*a.*) The presence of an alar membrane or patagium.
(*b.*) The presence of pterylæ and apteria.
(*c.*) The presence of remiges and of tectrices majores.
(*d.*) The attitude assumed during sleep.
(*e.*) The presence of two articular facets on the head of the quadrate.
(*f.*) The presence of a pygostyle.
(*g.*) The extreme variability of the sternum, shoulder girdle, and wing, indicating degeneration.
(*h.*) The occasional occurrence of a median longitudinal ridge or vestigial keel on the sternum.
(*i.*) The position of the shoulder girdle and sternum in stage E.
(*j.*) The presence of vestigial acromial, procoracoid, and acrocoracoid processes.
(*k.*) The fact that the skeleton of the fore-limb is that of a true wing in stage F.
(*l.*) The early assumption of undoubted avian characters in the pelvis.
(*m.*) The typically avian characters, both as to structure and development, of the vertebral column and hind-limb.
(*n.*) The fact that the brain passes through a typical avian stage with lateral optic lobes.
(*o.*) The relations of the subclavian muscle.

On the other hand, the total absence of rectrices tells against this view.

The following characters indicate derivation from a more generalised type than existing birds:—

(*a.*) The characters of the chondrocranium, especially in the earlier stages. Many of these peculiarities, *e.g.*, the absence of an interorbital septum, may, however, be adaptive, and correlated with the diminished eyes and the enlarged olfactory organs.
(*b.*) The presence of an operculum in early stages. As, however, this structure has not been described in Reptiles, it either proves nothing or too much.
(*c.*) The presence of a well-marked procoracoid in comparatively late embryonic life.
(*d.*) The characters of the pelvis.

On the other hand, in the following characters, *Apteryx* exhibits greater specialisation than other birds:—

(*a.*) The early assumption of their permanent position by the limbs.

(*b.*) The late appearance and obviously degraded character of the hyoid portion of the tongue-bone.

(*c.*) The position of the nostrils and the peculiar mode of development of the respiratory section of the nasal chamber.

(*d.*) The total absence of clavicles.

Such characters as the position of the basi-pterygoid processes, the broad vomer, and the presence of Jacobson's cartilages, being paralleled in existing Carinatæ, some of them even in Passerines, can hardly be considered as of fundamental importance, since they may be derived from a proto-carinate or from an early typical carinate stock.

Before considering the peculiarities in the development of the sternum as of fundamental importance, it will be necessary to study that of the flightless Carinatæ, and especially of *Stringops*.

The general balance of evidence seems to point to the derivation of both Ratitæ and Carinatæ from an early group of typical flying birds or *Proto-Carinatæ*.

IV. "Notes on some peculiar Relations which appear in the Great Pyramid from the precise Measurements of Mr. Flinders Petrie." By Capt. DOWNING, R.A. Communicated by Sir F. ABEL, F.R.S. Received March 13, 1890.

Presents, April 17, 1890.

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April 24, 1890.

Sir G. GABRIEL STOKES, Bart., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On a Pneumatic Analogue of the Wheatstone Bridge." By W. N. SHAW, M.A., Lecturer in Physics in the University of Cambridge. Communicated by LORD RAYLEIGH, Sec. R.S.

Received March 31, 1890.

When fluid flows steadily through an orifice in a thin plate, the relation between the rate of flow, V, measured in units of volume of fluid per second, and the head H (the work done on unit mass of the fluid during its passage) may be expressed by the equation :—

$$H = RV^2,$$

where R is a constant depending upon the area of the orifice, If the head be measured in gravitation units, R is equal to $1/2gk^2a^2$, where g is the acceleration of gravity, a the area of the orifice, and k the coefficient of contraction of the vein of fluid, a factor which is independent of the rate of flow.

Let us suppose a current of *incompressible* fluid to be drawn in succession through two orifices, a_1, a_2, arranged one at each end of a closed space, B, so large that there is no appreciable difference of head between different parts of it and that the kinetic energy of the flow through the one orifice does not affect the flow through the other. By the principle of continuity, the flow V will be the same through each of the orifices, and we have for the head H_1 between the two sides of the orifice of entry, $H_1 = R_1V^2$, and for the head H_2 between the two sides of the orifice of exit, $H_2 = R_2V^2$, where R_1 and R_2 are corresponding constants for the two orifices. From the definition of the term "head," it follows that $H_1+H_2(=\mathfrak{h})$ is the total head between the outside of the second orifice and the outside of the first. We may therefore regard H_1 and H_2 as partial heads which make up the total head $\mathfrak{h}$. We may suppose that the head $\mathfrak{h}$ is due to a constant manometric depression maintained in a second large closed space, A, communicating with the first space, B_1, by means of

the second orifice. If now we have a third closed space, B_2, likewise provided with two orifices, a_3, a_4, one of which, a_4, communicates with the space A where the constant manometric depression is maintained, while the other, a_3, is open to the same supply of fluid as that which feeds a_1, we get a second flow, V′, which we may speak of as being in multiple arc with the first, and to which the following equations apply:—

$$H_3 = R_3 V'^2,$$

$$H_4 = R_4 V'^2,$$

$$H_3 + H_4 = \mathfrak{h}.$$

H_3 and H_4 are the partial heads for the second flow, and R_3, R_4 the constants for the orifices a_3, a_4 respectively.

We have, therefore, an arrangement for the flow of fluid analogous to the arrangement of the Wheatstone quadrilateral for the flow of electricity, the galvanometer circuit being supposed open. The head $\mathfrak{h}$ corresponds to the electromotive force of the battery, V^2 and V'^2, correspond to the electric currents in the two branches; R_1, R_2, R_3, R_4, to the four electrical resistances; the spaces A, B_1, B_2, take the places of the brass connecting blocks of a Post Office box or the copper connexion pieces of a metre bridge. The partial heads, H_1, H_2, H_3, H_4, correspond to the electromotive forces between the ends of the four several wires. Making contact with a key in the galvanometer circuit would correspond to opening a tube of communication between the spaces B_1, B_2, above mentioned, and the hydrodynamic condition corresponding to no current through the galvanometer would evidently be the condition of no flow of fluid through the tube, and the galvanometer must be represented by some apparatus for detecting a flow of fluid; the detector need not, however, be designed to measure a flow any more than the galvanometer need be suitable for measuring a current. The condition for no flow in the "galvanometer" tube is that there should be no head between its ends; this condition is satisfied if $H_1 = H_3$ or $H_2 = H_4$; from which it follows that the condition is entirely independent of the total head, $\mathfrak{h}$, and depends only on the constants of the four orifices; we have, in fact, the ordinary Wheatstone-bridge relation:—

$$\frac{R_1}{R_2} = \frac{R_3}{R_4}.$$

If the coefficients of contraction may be assumed to be independent of the shape of the orifice, we get the condition for no flow through the "galvanometer" tube:—

$$\frac{a_1}{a_2} = \frac{a_3}{a_4},$$

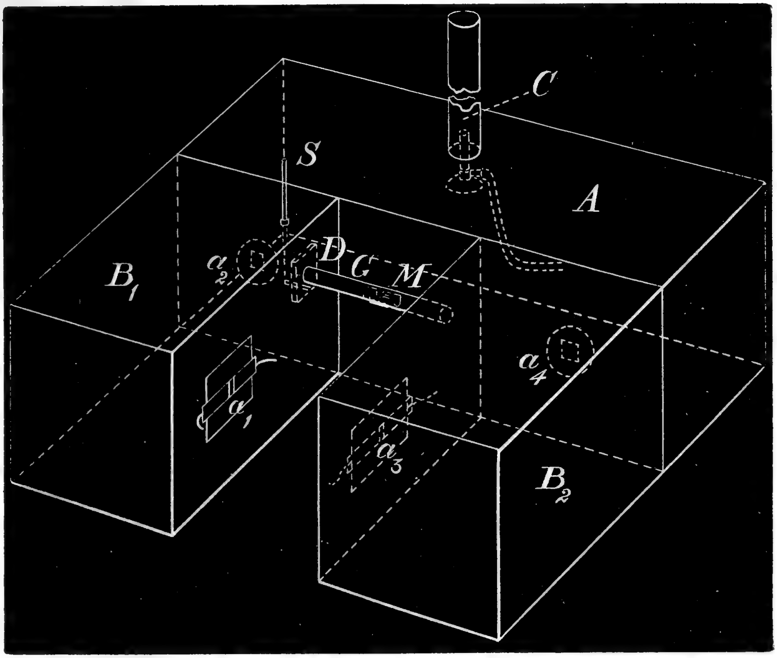
where the a's represent the actual measured areas of the four orifices.

It is evident that the practical realisation of this hydrodynamic analogue is by no means difficult. We require simply a "source" and a "sink" communicating, the one with the other, by two pairs of apertures in two separate boxes, which must be of such a size that the kinetic energy of the entering streams is practically completely dissipated in the boxes. The two boxes must also be connected by a tube in which there must be placed an apparatus for detecting the existence of a flow between the boxes.

The hydrodynamic analogue suggested itself to me in the course of the study of a number of problems in ventilation depending upon the flow of air between nearly-closed connected spaces, for example, adjoining rooms. In such cases the differences of pressure which produce the flow are very minute, amounting perhaps to a few hundredths of an inch of water, and the corresponding variations in the density of air may be safely disregarded. Under such circumstances the air will follow the laws of flow of an incompressible fluid, and equations identical with those quoted above will hold for the flow of air.

Measurements made upon the flow of air in order to determine the coefficient of contraction have been hitherto such as may be termed "absolute"; that is to say, the head and the flow have each been separately expressed in absolute measure and the value of R determined by taking the ratio of the head to the square of the flow. This process is exactly analogous to measuring the electrical resistance of a wire by finding the electromotive force between its ends and the current which flows along it.

M. Murgue, in a work on 'The Theory and Practice of Centrifugal Ventilating Machines' (translated by A. L. Steavenson), has shown that the internal resistance of a centrifugal fan to the flow of air through it can be calculated from the effects produced on the flow by varying the size of a second orifice through which the air has to pass. This process is evidently parallel to calculating the internal resistance of a battery by finding the effect produced upon the current by varying the external resistance. The development of the electrical analogy seems to afford a novel method of comparing resistances to the motion of air, and of verifying the laws of flow, and one which requires only a detector and not an anemometer, and is independent of the constancy of the flow. Whether it could be used practically to test the laws of flow and measure the pneumatic constants for various orifices to a higher degree of accuracy than has hitherto been attained, evidently depends upon the sensitiveness of the arrangement. In order to try this, I have had constructed what may be called a pneumatic analogue of the Wheatstone Bridge. It is represented in fig. 1, and consists of three wooden boxes, A, B_1, B_2.



A is 4 ft. × $1\frac{1}{2}$ ft. × $1\frac{1}{2}$ ft., and B_1 and B_2 are each 3 ft. × $1\frac{1}{2}$ ft. × $1\frac{1}{2}$ ft. The ends of B_1 and B_2 abut against the side of A, as shown in the figure; between B_1 and A is a rectangular opening, a_2, 1 in. × $\frac{1}{2}$ in., in a cardboard diaphragm, and between B_2 and A a rectangular opening, a_4, 1 in. × 1 in., in a similar diaphragm. In the side of B_1 at a_1 is an adjustable slit, made by cardboard shutters sliding in cardboard grooves, and at a_3 in the side of B_2, opposite to a_1, is a similar adjustable slit. The tube connecting B_1 and B_2, or "galvanometer" tube, is a straight tube of glass, G, of about 1·1 inch internal diameter. It can be closed at one end by a small trap-door, D, in the interior of the box B_1, which can be opened and shut by a steel wire, S, passing through a cork in the top of B_1. The sensitiveness of the apparatus depends upon the indicator employed. There are many indicators that might be employed; the one I have tried and have found to work well consists of two very small parallel sewing needles, stuck through a cap of elder-pith, supported on a small agate compass centre; the needles carry very light mica vanes on one side of the centre, counterpoised by a small quantity of platinum wire. The whole is balanced on the point of the finest needle I could obtain, and forms a very delicate wind vane. When first mounted, the needles always took up a position of equilibrium with the points

northward, although they had not been intentionally magnetised, nor, indeed, exposed to any risk of their being so from the time of their being purchased. They were, no doubt, very slightly magnetised, but the time of swing was very long, and the position of equilibrium not sufficiently definite. I therefore magnetised them more strongly; the little vane then took up, in consequence, a definite position of equilibrium with the planes of the vanes approximately north and south. The apparatus being so placed that the tube, G, is east and west, the vanes always set across the tube when there is no current. The needle points enable the position of equilibrium to be clearly identified by the aid of a fiducial mark on the glass tube. The sensitiveness can be altered as desired by an external control magnet, just as that of a galvanometer needle can be. The little compass needle or wind vane, M, is very sensitive to the motion of air in the tube, and although it may be possible to find other detectors that are equal, or even superior to it, yet the ease of seeing it, the rapidity of its action, and its definite zero are decidedly in its favour.

The head is produced by a gas burner in a metal chimney, C, fitted to the lid of the box A.

Various precautions are required in fitting the boxes together to secure that the air should only flow through apertures intended for its passage, but they need not here be detailed. They consist mainly in the plentiful application of glue and brown paper.

The apparatus was designed when I was making a number of observations of flow of air to illustrate the theory of ventilation, and I did not anticipate that any high degree of accuracy could be aimed at. I was, therefore, agreeably surprised to find that the identification of the condition of no flow is capable of much greater accuracy than the arrangements for measuring the areas of the orifices would allow me to interpret.

Of the four apertures of the bridge, two, viz., a_2 and a_4, are inaccessible without pulling the arrangement to pieces; they represent areas of $\frac{1}{2}$ sq. in. and 1 sq. in., respectively, as accurately as a knife could cut them in cardboard.

The other two areas, viz., a_1 and a_3, are made by sliding shutters, as already mentioned. Their edges were cut with a knife, and they probably are only rough approximations to areas in a truly thin plate, so that little importance can be attached to the final results of the measurements which will be given below; they serve only to show that the width of the adjustable slit, when there is no flow through the galvanometer tube, is a perfectly definite magnitude.

The following observations have been taken with the apparatus:—

I. *To Verify the Law of Proportionality of Areas, viz.*, $\frac{a_1}{a_2} = \frac{a_3}{a_4}$.

As already stated, $a_3 = 0{\cdot}5$ sq. in., $a_4 = 1$ sq. in., so that if the law holds a_3 should be found to be equal to $2a_1$. In testing the proportionality, a_1 was made successively equal to $\frac{1}{4}$, $\frac{1}{2}$, $\frac{3}{4}$, 1 sq. in., by the use of a cardboard wedge, 343 mm. length of which corresponds to 1 in. breadth; and a_3 was adjusted till there was no flow through the galvanometer tube, and the width measured by means of the cardboard wedge.

The measurements were referred directly to an inch scale by parallel-jaw callipers.

The observations are contained in the following table:—

Table I.

Area of a_1 in square inches	·25.	·50.	·75.	1·00.
Observation of width of a_3 in divisions of the wedge	165 165 166 166 165	337·5 (?) 333·5 333·5 333·5 334	495·5 496 495 494·5 494·5	[68·5]* [68·5] [69·0] [68·0] [67·0]
Means..................	165·4	334·4	495·1	[68·2]
Equivalents in square inches	·465	·985	1·46	1·92
$\frac{a_3}{a_1}$....................	1·86	1·97	1·95	1·92

The differences in the ratios a_3/a_1 for different values of a_1 are considerable, but it must be remembered that the greatest fractional difference, being 1/19th of the whole, would be accounted for by an error of 1/76th of an inch in the adjustment of a_1 to $\frac{1}{4}$ inch, and a cardboard slit, cut with a knife, can hardly be expected to reach beyond that limit of accuracy. It is evident, from the readings given, that the condition of no flow is capable of very accurate experimental definition with the little compass detector.

II. *Verification of the Inference that the Condition of No Flow is Independent of the Total Head.*

This has only been carried so far as to determine whether, when the adjustment of areas was made, the equilibrium could be dis-

* The readings in this column were taken by means of a different and wider wedge.

arranged by altering the quantity of gas burning in the jet. No difference was, however, observed in the position of equilibrium of the needle, whether the gas was quite low, or on full, or turned out, leaving only the head due to the heat of the metal chimney. So far as could be tested in this manner, one of the advantages of the Wheatstone bridge, viz., that the adjustment is independent of the electromotive force, is correctly followed in the pneumatic analogue.*

III. *Comparison of a Circular with a Rectangular Aperture.*

A circular aperture in a brass plate, one-sixteenth of an inch thick, was balanced against a rectangular one, formed by the sliding card-board shutters. The circle was turned to be 1 inch in diameter, and the inner edge of the aperture was bevelled. The observations, when the circle was in the position a_1, were—

$$a_1 = \pi \times (\tfrac{1}{2})^2 = \cdot 785 \text{ sq. in.}$$

Readings for a_3, $\left\{\begin{matrix} 489\cdot 5 \\ 490 \\ 490 \end{matrix}\right\}$ Mean, 490.

Whence $a_3 = 1\cdot 446$ sq. in.

$$\frac{a_3}{a_1} = 1\cdot 86. \qquad \frac{a_4}{a_2} = 2.$$

When the circle was in the position a_3,

$$a_3 = \cdot 785 \text{ sq. in.}$$

Readings for a_1, $\left\{\begin{matrix} 125 \\ 126 \\ 125\cdot 5 \end{matrix}\right\}$ Mean, 125·5.

Whence $a_1 = \cdot 362$ sq. in.

$$\frac{a_3}{a_1} = 2\cdot 17. \qquad \frac{a_4}{a_2} = 2.$$

The observations were repeated with similar results.

These two values of the ratio a_3/a_1 would be reconciled by assuming that the circular aperture was only equivalent to a rectangle whose area is 0·925 of the circular aperture, and they, therefore, throw doubt upon the idea that circular and square apertures have the same coefficient of contraction, but the rectangular apertures were

* The flow of air through an aperture (a_3) of 1 sq. in. amounts to about 2 cubic feet per minute when the gas is very low, and to 4 cubic feet per minute when it is full on, so that the head can be changed in a ratio of about 4 : 1.

not such close approximations to orifices in truly thin plates as to warrant the acceptance of this result without apparatus of more elaborate construction. Moreover, there is a possibility of slight leak in the grooves of the shutters, which ought not to be disregarded.

The observations are, however, sufficient to show that a properly constructed apparatus is capable of making measurements of the effective areas of orifices with a very considerable degree of precision. It is well known that if the orifice be not an aperture in a thin plate, but in the form of a tube, straight or bent, the flow through the orifice can be represented by an equation of the same form as if the orifice were a thin plate aperture, viz.:—

$$H = RV^2,$$

but in the case of a more complicated orifice R cannot be so easily calculated from the dimensions; the value of R might, however, be determined experimentally for an orifice of any shape and dimensions by a pneumatic bridge of suitable size, and the result might be expressed, as M. Murgue suggests for the case of mines in the work already referred to, by stating the area of the thin plate orifice to which the given orifice is equivalent. The comparison of calculated values of R with observed values obtained by a pneumatic bridge would enable us to determine a number of pneumatic constants that are at present only comparatively roughly ascertained, such, for instance, as the coefficient of air friction in tubes of different diameters, the constants of different forms of orifice, the effect of bends and elbows in pipes, and of gauze or gratings covering an orifice. And it would not, I think, be difficult to arrange the apparatus in such a way as to determine the law of resistance of a disc to the passage of air and its variation with velocity. The velocity can be increased to any extent that may be necessary by using a centrifugal fan to produce the head instead of the gas burner.

I am intending, if possible, to have my present apparatus altered in some of its details, so that the orifices may be more definitely expressed in terms of thin plate apertures, and then to use it for the determination of some of the pneumatic constants I have referred to.

II. "On the Effect of Tension upon Magnetic Changes of Length in Wires of Iron, Nickel, and Cobalt." By Shelford Bidwell, M.A., F.R.S. Received April 8, 1890.

Preliminary.

A former communication to the Royal Society ('Roy. Soc. Proc.,' No. 243, 1886, p. 257) contains an account of some experiments relating to the magnetic extensions and contractions of iron wires

under tension. Wires of several different sizes and qualities were suspended inside a magnetising coil, and were loaded with various weights; and in each case observations were made of (1) the smallest magnetising current which caused sensible change of length; (2) the current producing maximum elongation (if any) and the value of such elongation; (3) the critical current which was without effect upon the length of the wire; and (4) the contraction produced by a certain strong current.

The results indicated that the maximum elongation became smaller as the load was increased, disappearing altogether when the tension exceeded a certain limit; and that contraction began to take place at a correspondingly earlier stage in the magnetisation.

These results were chiefly of interest as disproving Joule's conjecture, which has often been quoted as if it were an experimental fact, that, under certain critical tension (differing for different specimens of iron, but independent of the magnetising force*), magnetisation would produce no change whatever in the length of the wire.

The subject, however, seemed worthy of more complete investigation, and I have lately undertaken a series of experiments in which the changes of length undergone by a stretched iron wire were traced continuously as the magnetising force was gradually increased from a small value up to about 375 C.G.S. units. Similar experiments were also made with a nickel wire and with a thin strip of cobalt, the behaviour of these metals under tension never having been previously studied.

Apparatus.

The apparatus employed was the one described and figured in my former paper. The diagram there given, together with a short description, is, for convenience, here reproduced (see fig. 1).

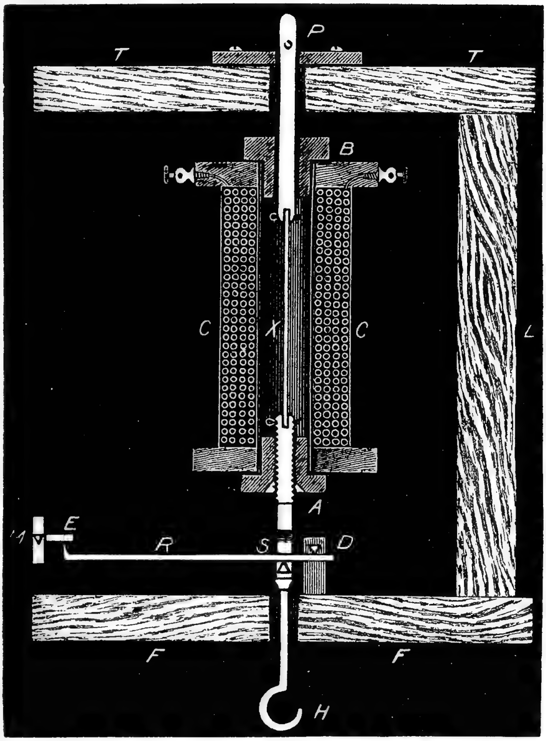
Experiments.

For reasons which need not be repeated here, it was found necessary to support the magnetising coil in the manner shown in the figure, its whole weight being borne by the experimental wire. The minimum load on the wire was therefore represented by the weight of the coil, together with the pull of the lever, the two amounting to 1·36 kilo. Greater tension was produced by attaching weights to the hook H.

The *iron* used was a piece of soft annealed wire, 0·7 mm. in diameter and 10 cm. in length, between the clamps. The weights

* At least within the limits of the forces employed by Joule, estimated to range from 7 to 114 C.G.S. units. See 'Roy. Soc. Proc.,' No. 242, 1886, p. 112.

Fig. 1.



The magnetising coil, CC, is supported by a stopper, A, which is inserted into the bottom of the coil. Through an axial hole in A is screwed a brass rod, terminating in a stirrup, S, beneath which is fixed a hook, H, for the suspension of weights. A second brass rod, suspended by a pin at P, passes freely through a stopper, B. The wire under experiment, X, is clamped between the ends of the brass rods. The knife-edge at the bottom of the stirrup acts upon a brass lever, R, one edge of which turns upon the knife-edge D, the other actuating a short arm, E, attached perpendicularly to the back of the mirror M. The mirror turns upon knife-edges about its horizontal diameter. By means of a lantern the image of a fine wire is, after reflection from the mirror, projected upon a distant vertical scale and serves as an index. Dimensions:—SD = 10 mm., DE = 170 mm., ME = 7 mm., distance from mirror to scale = 4706 mm., each scale division = 0·64 mm., length of X = 100 mm.

successively attached to it were equivalent to 1950, 1600, 1170, 819, 585, and 351 kilos. per square cm. of section.

The *nickel* wire was 100 mm. long, and 0·65 mm. in diameter; it was supplied by Messrs. Johnson and Matthey. The loads under which it was examined were 2310, 1890, 1400, 980, 700, and 420 kilos. per sq. cm.

The *cobalt* used was a narrow strip measuring 100 mm. by 2·6 mm. by 0·7 mm.; its cross section being, therefore, 1·82 sq. mm. It was not possible to obtain this metal in the form of a wire; and for the piece of thin rolled sheet from which the strip above-described was formed, I am indebted to the kindness of Messrs. Henry Wiggin and Co., of Birmingham, who had it specially prepared for me. The loads employed for the cobalt strip were equivalent to 772, 344, and 75 kilos. per sq. cm. The first of these was represented by an actual weight of 31 lbs.,* which was as great as the apparatus seemed capable of bearing without risk of injury.

In all the experiments the loads were successively applied in decreasing order of magnitude, and before every single observation the wire or strip was demagnetised by reversals, without, of course, being removed from the coil.†

The results obtained for iron are given in Table I, and also shown in the curves in fig. 2.

Those for nickel are given in Tables II and III, and in figs. 3 and 4. In fig. 3 the curve for 700 kilos. is represented by a dotted line for the sake of distinctness. Table III and fig. 4 are constructed from data obtained from a complete set of curves, like those in fig. 3; they show the magnetic contractions that would occur under increasing loads in constant magnetic fields of 125, 185, and 360 C.G.S. units respectively. These magnetic contractions would, of course, be superposed upon elongations of a purely mechanical nature, due to the tensional stress.

The results for cobalt are contained in Table IV and fig. 5. In the figure the contractions corresponding to the various loads are indicated by different kinds of marks, and a single curve has been drawn as smoothly as possible through the whole of them.

In all the tables and figures magnetic fields (which are those due to the coil alone) are given in C.G.S. units, and increments and decrements of length are expressed in ten-millionths of the length (10 cm., or about 4 inches) of the experimental wire or strip. In figs. 2 and 5, therefore, the height of each little square corresponds to 1/10,000 mm., or 1/250,000 inch, and in figs. 3 and 4 to 1/2,000 mm. or 1/50,000 inch.

* 14 kilos.

† The apparatus used for this purpose is described in 'Phil. Trans.,' vol. 179 (1888), A, p. 206.

Table I.—Iron.

Magnetic field in C.G.S. units.	Elongations in ten-millionths of length with loads per sq. cm. of					
	351 kilos.	585 kilos.	819 kilos.	1170 kilos.	1600 kilos.	1950 kilos.
7	2	2	0	0	0	0
9	..	..	1·5	0	0	0
11	6·5	8	..	0	0	0
16	14	12	8·5	2	− 1	− 2
22	20	18	11·5	2·5	− 2	− 2·5
35	27	23	14·5	3·5	− 3	− 4·5
50	26·5	23	13	2		
88	25	19	9	0	− 9	−13
138	17	10	2	− 7	−17	−24
188	10·5	3·5	− 3	−13·5	−23	−32
281	0	− 9	−18	−24·5	−37	−48
375	−9·5	−21	−28	−39	−52	−62

Table II.—Nickel.

Magnetic field in C.G.S. units.	Contractions in ten-millionths of length with loads per sq. cm. of					
	420 kilos.	700 kilos.	980 kilos.	1400 kilos.	1890 kilos.	2310 kilos.
13	2	2	2	0	0	0
16	4	3	2	0	0	0
19	12	5	4	0	0	0
28	30	9	8	1	2	0
34	43	22	15	2	3	2
50	69	40	27	4	5	2
69	..	..	..	15	8	
72	102	65	53			
84	..	..	..	..	..	7
88	123	96	73	26	12	
103	142	123	98	43	21	13
125	162	156	122	56	30	17
159	190	193	165	88	52	32
184	209	221	195	109	..	43
188	..	..	..	..	68	
219	232	245	237	152	..	60
225	..	..	..	..	94	
275	256	275	284	194	..	91
284	..	..	..	..	127	
359	..	315	334	242		
363	288	..	..	..	..	140
384	..	..	..	..	176	

Fig. 2.

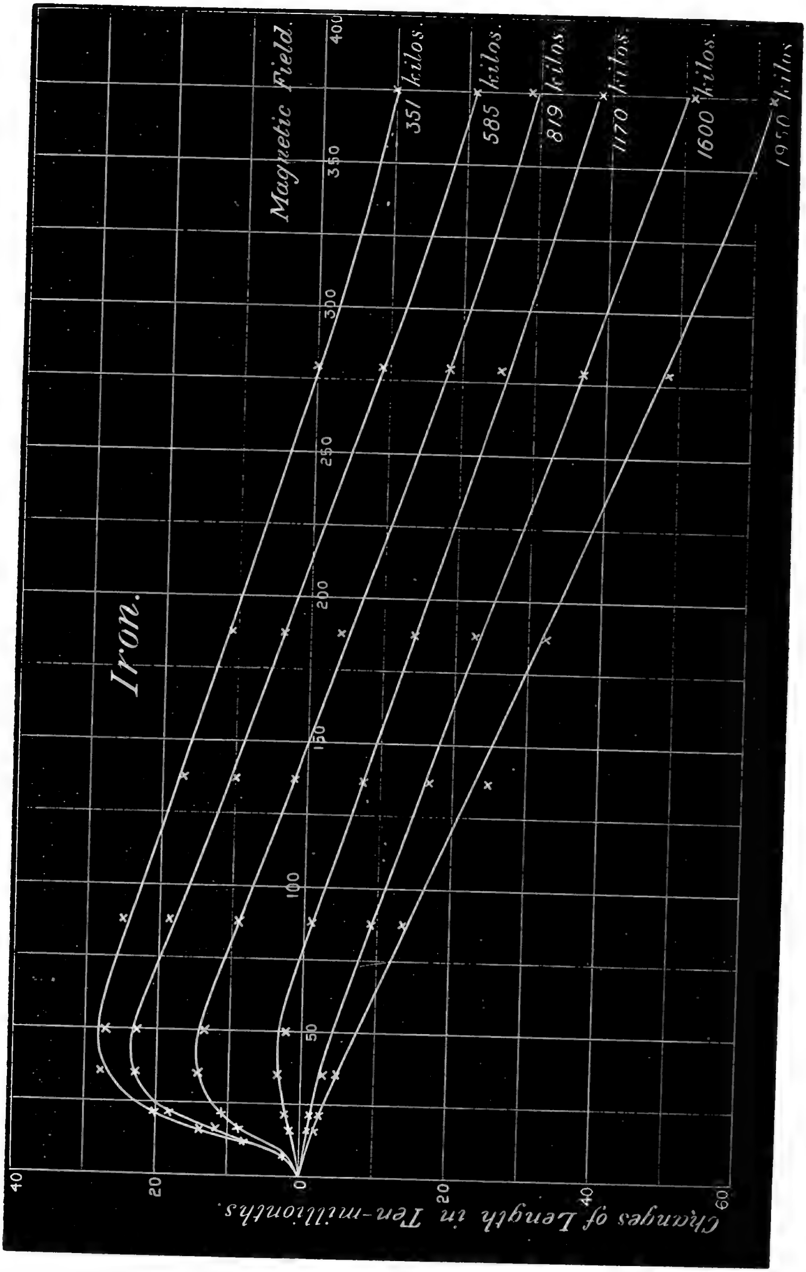

Iron.
Magnetic Field.
351 kilos.
585 kilos.
819 kilos.
1170 kilos.
1600 kilos.
kilos.
50
100
150
200
250
300
350
400
40
20
0
20
40
60
Changes of Length in Ten-millionths.

FIG. 3.

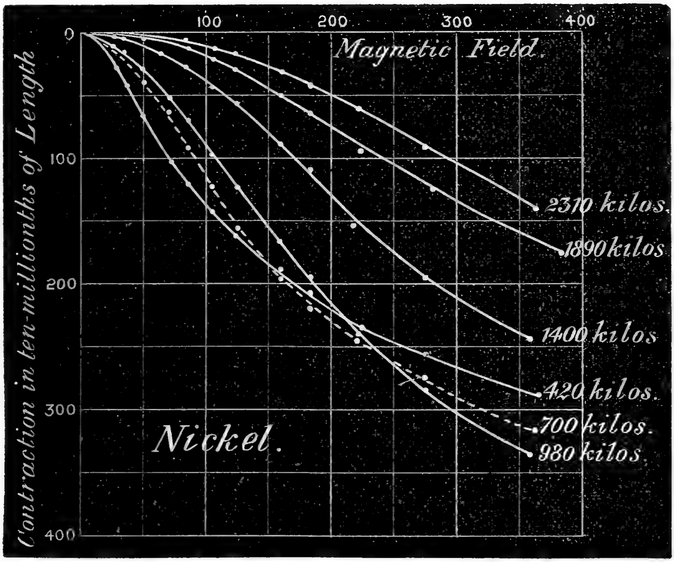

Magnetic Field.
Contraction in ten-millionths of Length
0
100
200
300
400
2310 kilos.
1890 kilos
1400 kilos
420 kilos.
700 kilos.
980 kilos.
Nickel.

FIG. 4.

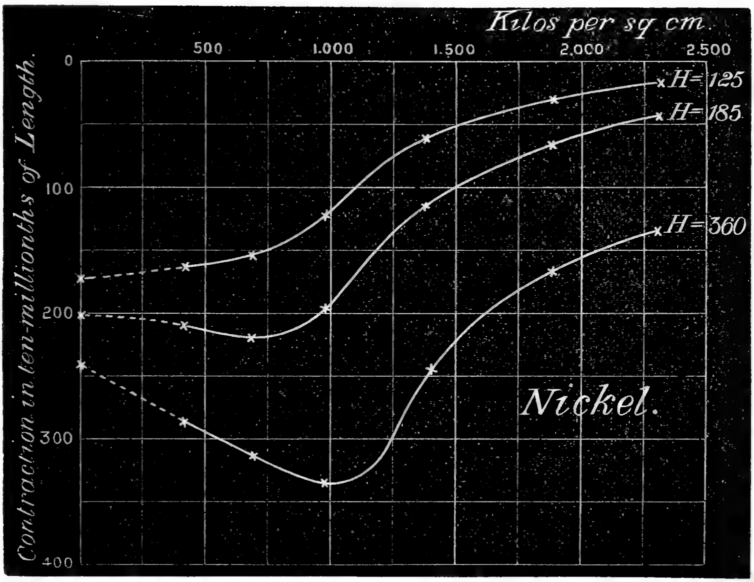
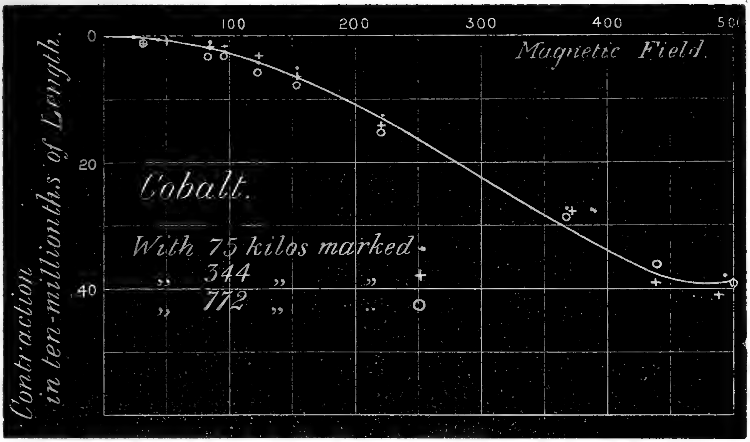

Kilos per sq. cm.
500
1.000
1.500
2.000
2.500
Contraction in ten-millionths of Length.
0
100
200
300
400
H = 125
H = 185
H = 360
Nickel.

Table III.—Nickel.

Load in kilos. per sq. cm.	Contractions in ten-millionths of length in fields of		
	125 units.	185 units.	360 units.
0	172	200	242
420	162	209	287
700	156	221	315
980	122	195	334
1400	56	109	242
1890	30	66	165
2310	17	43	136

Table IV.—Cobalt.

Magnetic field in C.G.S. units.	Contractions in ten-millionths of length with loads per sq. cm. of		
	75 kilos.	344 kilos.	772 kilos.
22	0	0	0
34	0·5	0·5	0·5
47	0·5	..	0·5
50	..	0·5	..
84	1·5	2	3·5
98	3	2	3·5
125	4·5	3·5	6
156	6	7	10
219	13	15	15·5
369	28	28	29
438	38·5	39	36
490	..	41	
493	38		
500	..	..	39·5



Discussion of the Tables and Curves.

Iron.—The curves in fig. 2 clearly show the effect of tension in diminishing the maximum elongation and hastening the contraction. With the two greatest loads used there was no preliminary elongation at all. The curve for an unstretched iron rod is generally found to cut the axis at about 300; probably, therefore, if the wire used in these experiments could have been tested with no load, its curve would lie a little above that for the load of 351 kilos.

Nickel.—The results for nickel are of great interest. In fields below about 140 units increase of load is always accompanied by decrease of magnetic contraction, the earlier portions of the six curves in fig. 3 following one another in inverse order of the magnitude of the several loads. But, although the initial slope of the curves diminishes with increasing loads, the "turning points," where the ratio of the contraction to the magnetic field is a maximum, occur later with great than with small loads, so that in a field of 360 the order of the relative values of the contractions for the three smallest loads is actually reversed, the contraction being greatest with the heaviest load and least with the lightest. It appears probable that, if the experiment had been carried far enough, the curve for 1400 kilos. would have crossed one if not all of the three curves lying below it. Whether the two remaining curves for 1890 and 2310 kilos. would behave similarly is more doubtful. Possibly they would have become parallel to the horizontal axis before the others were reached.

I think it may fairly be assumed that if the experiment could have been made with the wire quite unstretched, we should have obtained a curve having a steeper initial slope than any of those in the

diagram, but also reaching its turning point sooner, and therefore intersecting at least three or four of the others. Such an assumption is confirmed by some results obtained with a nickel rod 0·3 cm. in diameter, which have been given in a former communication,* and are reproduced in Table V.

Table V.—Unstretched Nickel Rod.

Magnetic field in C.G.S. units.	Contractions in ten-millionths of length.
65	104
125	167
181	199
237	218
293	233
343	240
393	242

A curve plotted from this table will be found to begin its descent between the curve for 420 kilos. and the vertical axis, and after crossing the others to intersect that for 1400 kilos. at $H = 355$. But of course experiments made with two different specimens of nickel are not strictly comparable.

In fig. 4 the results for nickel are presented in a somewhat different form, the curves showing the magnetic contractions of the wire under various loads in certain constant fields. The values for no load are taken from Table V, and the portions of the curves which depend upon the accuracy of these values are distinguished by dotted lines. It will be seen, however, that these portions are not of much importance.

From these curves we at once see that in a field of 125 units, increase of load always causes decrease of magnetic contraction. In a field of 185, magnetic contraction increases as the load is raised from nothing up to about 700 kilos. per square cm., again decreasing with greater loads. And in a field of 360 units this singular reversal is exhibited in a still more marked degree, the maximum magnetic contraction occurring with a load of about 950 kilos. per square cm.

The reversal phenomena observed in connexion with the magnetic contraction of nickel are strikingly analogous to those which occur in the magnetisation of a stretched iron wire, and which are commonly associated with the name of Villari.†

* 'Phil. Trans.,' A, 1888, p. 228.

† Poggendorff's 'Annalen,' 1868. See also 'Encycl. Britann.,' 9th edit., vol. 15, p. 269. When an iron wire subject to a magnetising force in the direction of its length is stretched by a certain force, the magnetisation of the wire is increased or diminished according as the magnetising force is less or greater than a certain critical value

Professor J. J. Thomson has shown* that the Villari effect is dynamically connected with the changes of length undergone by an iron rod when magnetised, an iron rod being lengthened in a weak magnetic field and shortened in a strong one. Now cobalt behaves oppositely to iron in this respect, a rod of cobalt becoming shorter in a weak field, longer in a strong one.† Professor Thomson, therefore, predicted that a Villari reversal would be found to occur in cobalt and that it would be of the opposite character to that in iron. Some experiments made by Mr. Chree‡ with a cobalt rod under pressure gave results in accordance with Professor Thomson's expectation.

But no reversal of the effect of magnetisation in diminishing the length of an unstretched nickel rod has ever been observed. Such a rod always becomes shorter in a magnetic field, whether strong or weak. It appears to attain its shortest length in a field of about 750 units, but it does not pass a minimum and become longer again in stronger fields, like cobalt. Applying Professor Thomson's reasoning, therefore, to the case of nickel, we should expect that it would be found not to exhibit any Villari reversal, either of the nature of that in iron, or of that in cobalt. Both Sir William Thomson and Professor Ewing have in fact looked for one and failed to find it. But in a paper read at the meeting of the Physical Society, on 21st March, 1890,§ Mr. Herbert Tomlinson gave an account of some experiments which, as he believed, showed that a Villari critical point really existed in nickel, though it was only to be reached by the application of comparatively great magnetising forces. If Mr. Tomlinson is right, I venture to suggest that his results may possibly be brought into harmony with Professor J. J. Thomson's mathematics by consideration of the experiments described in the present paper: for, as I understand Professor Thomson's argument, he has hitherto taken no account of the effects of mechanical stress upon magnetic changes of length.

Cobalt.—The results for cobalt show that the changes of length which this metal undergoes when magnetised are almost, if not entirely, unaffected by tensional stress, at least within the limits of the experiments. Having regard to the very marked influence of tension upon iron and nickel, this cannot but be regarded as a most remarkable fact.

(depending upon the magnitude of the stretching force) for which stretching produces no effect.

* 'Applications of Dynamics to Physics and Chemistry,' p. 54.

† 'Phil. Trans.,' A, 1888, p. 227.

‡ *Supra*, p. 41.

§ Not yet published.

Summary of Results.

Iron.—Tension diminishes the magnetic elongation of iron, and causes contraction to take place with a smaller magnetising force.

Nickel.—In weak fields the magnetic contraction of nickel is diminished by tension. In fields of more than 140 or 150 units, the magnetic contraction is increased by tensional stress up to a certain critical value, depending upon the strength of the field, and diminished by greater tension.

Cobalt.—The magnetic contraction of cobalt is (for magnetic fields up to 500 C.G.S. units and loads up to 77·2 kilos. per sq. cm.) practically unaffected by tension.

III. "On the Heat of the Moon and Stars." By C. V. Boys. A.R.S.M., F.R.S., Assistant Professor of Physics, Normal School of Science and Royal School of Mines, London. Received April 14, 1890.

Soon after I had completed the radio-micrometer and shown its great superiority over any form of thermopile and galvanometer, I was naturally anxious to carry out some research which would clearly demonstrate the capabilities of the instrument. The determination of the heating powers of the stars seemed most promising, for Dr. Huggins had, in 1869,* made experiments on the heating powers of some of the stars which, though they did not conclusively show that a thermopile was capable of measuring so minute a radiation, yet made it exceedingly probable that the effects observed, if not very exact in quantity, were at any rate real. Dr. Huggins, however, described his experiments and formed his conclusions with the utmost caution. A year later Mr. Stone described experiments which he had made with the great equatorial at Greenwich.† He at first used small thermopiles, but soon found, as we should expect, that a single pair was more sensitive to radiation brought to a point than a pile of many pairs. In attempting to obtain great sensibility by giving the galvanometer a long period he found it almost impossible to use the apparatus on stars at night. Every slight change in the sky, even though quite invisible to the eye, so disturbed the galvanometer that it was impossible to distinguish effects due to the stars from those caused by the varying clearness of the sky. Mr. Stone largely obviated this difficulty by placing in the focal plane of the object glass a couple of thermo-electric pairs so connected that a heating of the exposed face of one would produce an effect opposite

* 'Roy. Soc. Proc.,' vol. 17, p. 309.

† *Ibid.*, vol. 18, p. 159.

in kind to that produced by a heating of the exposed face of the other. Under these conditions a change in the sky which would act on both faces alike, or nearly so, would not disturb the galvanometer, whereas a star made to shine first on one face and then on the other would cause a deflection first in one direction and then in the other. This arrangement had previously been employed by Lord Rosse, in his experiments on the heat of the Moon.* The pairs used by Mr. Stone were about 31 mm. long and had a sectional area of about 4 square mm., that is, each bar was about $31 \times 2 \times 1$ mm. Wires were taken to a distant galvanometer and the telescope was set with the image of the star alternately on the two faces. About 10 minutes were allowed before a reading was taken. The rays of Arcturus concentrated by the $12\frac{3}{4}$-inch object glass produced deviations of from 20—30 divisions of the scale, while a 3-inch cube of boiling water at two feet from the faces produced a deflection of about 150 divisions. Mr. Stone concluded that the face of the pile was heated through about 1/50th of a degree Farenheit. With these figures before me, I had no doubt that the radio-micrometer, which in sensibility vastly exceeds the thermopile, while unlike the thermopile and galvanometer it is free from disturbing effects of magnetism and outside changes of temperature and has the further advantage —and for astronomical work this perhaps is even more important— that a measure can be made in *five seconds* instead of several minutes which are necessary with the older apparatus, would be capable not only of making good and exact measures of the heat of the brighter stars, but I went so far as to hope that even faint stars would produce an appreciable effect and that most interesting results might be derived from an examination of planets, comets, nebulæ, and the red stars.

I therefore determined to put the radio-micrometer to a severe test, and one which promised not only to show its suitability for such delicate work, but at the same time to give much valuable information. The Royal Society gave me, out of the Government Grant, a sum of £50, which, thanks to the advice which I received, especially from Mr. W. H. Massey, and Mr. A. A. Common, in the matter of design and construction, was nearly sufficient to meet all the expenses which I have incurred. I should say also that Mr. Paxman, of Colchester, who made the steel tube, which is a beautiful example of miniature boiler construction, kindly presented this in the cause of science; that I have been able to use some few pieces of apparatus belonging to the Physical Laboratory, at South Kensington, such as the large magnet of about 25 lbs. for the radio-micrometer, some of the lime-light apparatus, and the finder; and,

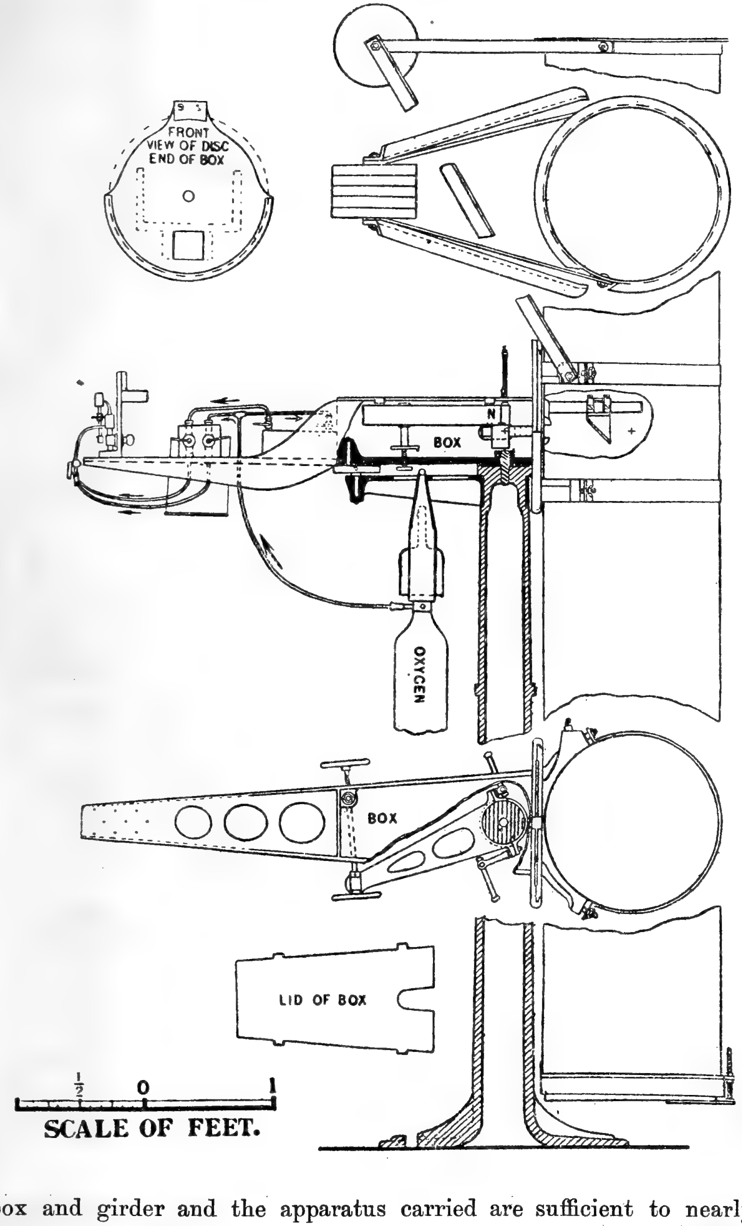
* 'Roy. Soc. Proc.,' vol. 17, p. 436.

finally, that Dr. Huggins has kindly lent me a silver-on-glass mirror of nearly 16 inches aperture and very short focus (67·8 inches) of his own construction, which was exactly what I wanted for this purpose. I accordingly prepared a design for a mounting of this mirror, such that, no matter in what direction the telescope might be pointed, the focus of the rays from a star should be always at one fixed point at which the receiving surface of the radio-micrometer could be suspended. I should say here that in this one particular the radio-micrometer is at a disadvantage by comparison with a thermopile or bolometer; it must not be tilted, it cannot be fixed into an eyepiece and pointed and moved about at will; it must be level, though it may, with its lamp and scale, be carried on a level platform and turned about a vertical axis. This disadvantage, which, however, disappears when the whole apparatus is designed for and is made to suit the radio-micrometer, is more than counterbalanced by the absence of connecting wires with a thermo-electric junction at every binding screw and by the absence of the galvanometer, which is ever ready to give indications on its own account.

In the design of the mounting I have, to a large extent, been obliged to follow certain lines. Thus it is evident, if a large siderostat is not to be had so that the telescope may be fixed, which would be the most convenient plan to adopt, that the mounting must be altazimuth, that the horizontal and vertical axes of motion must intersect, and that the focus, which must be just outside the tube, must be on or close to the vertical axis. With this arrangement trunnions to the telescope would be very inconvenient, as they would necessitate a long and awkward curved arm, which would prevent the telescope from being turned over from east to west, or from north to south. Accordingly, I have adopted the plan suggested by Mr. W. H. Massey, of carrying the telescope by a large disc on its side, resting and turning in an under-cut groove. Fig. 1 shows the chief details of construction finally determined upon, and fig. 2 the finished instrument in position. The radio-micrometer must be protected from stray radiation and from the effects of hot and cold air currents. I therefore arranged that the space at the back of the under-cut disc should be made in the form of a box with a movable lid, all of thick cast-iron, so that the changes of temperature inside should be sufficiently gradual; then the nature of the radio-micrometer would prevent such changes from producing any disturbing effect. The floor of this box continued away from the telescope forms a convenient base for holding the lamp (ether oxygen lime light) and scale.

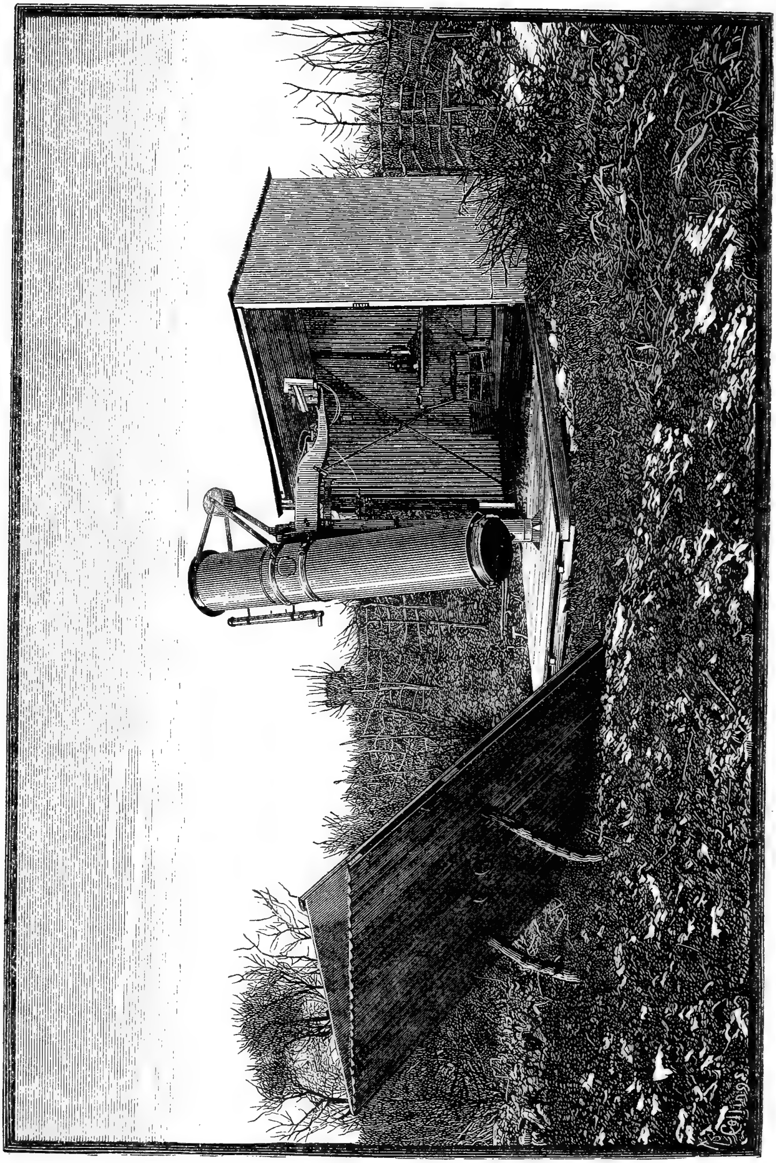
The telescope tube carried by the disc on its side must be balanced about the centre of this disc. The large cast-iron weights carried by four rods are in such a position as to balance it exactly; moreover the

Fig. 1.



box and girder and the apparatus carried are sufficient to nearly balance everything on the head of the vertical column, and so relieve the holding-down bolt of all strain.

FIG. 2.



The vertical column appears too slender. It was, however, important to get the focus as near the side of the telescope as possible, and, as the focus should be on or close to the vertical axis, it is clear

that the column must be as slender as possible so long as its stability is not impaired. The further the focus is outside the telescope, the larger must be the plane mirror. As this is a disadvantage both on account of expense and of obstruction of useful rays, I have materially reduced the size of the flat by placing it well on one side of the axis. The resulting want of definition I found by calculation to be for my purpose of no importance. So long as all the rays fall on the sensitive surface, which in some cases is 1 mm. square, the definition is sufficiently good. With the very low power eyepiece that I use I cannot detect anything more than a point in the image of a star.

The radio-micrometer which I made for this purpose is unusually large and massive, and, as the suspended circuit is hung in a narrow hole drilled in this great mass of solid metal, differences of temperature in different parts of the circuit can hardly be produced by outside influence, as, for instance, the observer's body. Any heat so applied must first unequally warm the massive cast-iron box; it must then be imparted to the solid metal radio-micrometer (nearly 30 lbs.), which is only supported by five points forming a geometrical slide, and then it will only be the difference of temperature in the solid metal between points not much more than a quarter of an inch apart, which by imparting some portion of itself to the suspended circuit will cause any indication of heat or cold. If, however, radiation reaches the sensitive surface through the small hole drilled horizontally in the solid metal, and this can only come from the limited field of view of the telescope, then one portion of the circuit will be independently warmed, and a corresponding deflection will be produced.

The only part of the instrument which reaches outside the cast-iron box is a slender tube which carries the cork and pin from which the circuit is suspended by a very fine quartz fibre. This tube is made of glass ground into the metal of the radio-micrometer. The object of using glass is to prevent any loss of heat from the apparatus into the outer air. This part is also boxed in by an easily removable double box of wood. In making this tube I blew a hole in the side and thickened it with a welt of glass about half an inch above the level of the box lid. To the face of the welt previously ground flat is cemented a piece of a plano-convex spectacle lens, which forms an image of its own on the scale and at the same time brings the light reflected from the plane mirror behind it to a focus on the same scale. The mirror, which consists of a piece of the thinnest microscope cover-glass silvered at the back, produces with this arrangement an image so good that tenths of millimetre can be easily read. To prevent the delicate circuit from being influenced by draughts in the telescope, a tube containing diaphragms is fastened

to it and projects nearly as far as the side of the telescope tube. The diaphragms are of such a size as to limit the view from the sensitive disc to the cone of rays. A tube of this kind was used by Langley to protect his bolometer from the influence of draughts. It is nothing more than the old toy through which you can drop a pencil but not blow out a candle.

The arrangements of lamp and scale are hardly worth describing in detail. I will merely say that I used the ether light to avoid the necessity of having two kinds of compressed gases, which, in the country, would otherwise be necessary. The ether light is exceedingly convenient for this purpose, but I can hardly recommend it, for, though I used the safety burner supplied by the makers, the first thing it did, owing to my inadvertently turning off the oxygen, was to explode with a loud report, and the copper box, after striking the roof of the house, fell close to me, not burst, it is true, but blown into a more or less bulbous form. Moreover, on frosty nights, the ether box is so cold that the gas which comes out requires no more oxygen, so that an explosive gas is being led from the reservoir direct to the burner. Under these circumstances any stoppage of the oxygen supply would at once cause a violent explosion. As it is, it is impossible, on a cold night, to stop the gas without its exploding down to the tap which turns it off.

I provided, between the ether box and the burner, a pair of extra regulating taps, which, by a touch, will turn off the oxygen and turn down the (so-called) hydrogen, or turn them up again for an observation. By this means waste of ether and oxygen is avoided, and the limes last a long time.

I have arranged a slow motion in azimuth, which is more convenient when observing on or near the meridian, but none in altitude, as that would have been troublesome to make. The motion in altitude is rather stiff, but being used to it now I can follow a star in any part of the sky, step by step, without difficulty.

As I have already said, the tube was made by Messrs. Davy, Paxman, and Co., of Colchester; the stand and heavy fittings were made by Messrs. Thomas Horn and Sons, of Gray Street, Waterloo Road, S.E., engineers, and with regard to this part of the work, I must express my great satisfaction at the way the work has been carried out. Nothing is done for show, but every working surface is true, and works freely without shake. I believe this is the first thing of the kind that Mr. Horn has made; if he had done no other class of work but this he could not have done it better. The radio-micrometer and all the odd fittings and adjustments I made myself, and these parts have given no trouble. Into the details of the mounting of the mirror and certain minor adjustments it is not worth while to enter at length. It is sufficient to say that every part is capable of

independent adjustment, so that ultimately the focus is in the vertical axis of rotation, and at the same time the cone of rays from the large mirror is just not sufficient to cover the surface of the flat.

Though there is a finder, it would hardly be safe to trust to this to know when a star had just come on to the sensitive surface, and so I have arranged a 1-inch total reflecting prism behind the metal block under the magnet, which can be turned round so as to view the sensitive surface, and any image in the small space round it, from either side of the box. There is an oblong hole in each side for this purpose, through which a low power eyepiece, carried by a bracket on the metal block, projects; the space round the eyepiece is covered in by a separate shield, to prevent hot or cold air from entering the box. The dark radiations from the pupil of the eye are entirely prevented, by the three glasses, from reaching the sensitive surface, so that it is possible to watch the image of any heavenly body quietly transit across the disc or sensitive surface without disturbing the indications by the heat of the eye. I have arranged a temporary small telescope with a diagonal eyepiece, immediately above that of the chief telescope. The small telescope shows that part of the scale to which the spot of light is brought, magnified, so that without moving the body or any part of the apparatus it is possible to watch a star come on to the disc, and to see the effect on the scale, and thus to avoid every source of error at once. If in any case a star is observed to transit over the disc time after time, and the index is not moved through one-quarter of a millimetre (and I find on a perfectly clear and quiet night there can be no doubt whether this is so or not,—I should even have little doubt of a tenth of a millimetre), then it is certain that the heat received was not sufficient to produce such a deflection. An equatorial star takes about 20 seconds to cross the disc, while practically the whole deflection due to any source of heat is produced in 5 seconds, and so, no matter how long the star might be kept on there would be no gain, while, on the other hand, the longer that it is necessary to leave the star on before practically the whole deflection is produced, the greater is the uncertainty of the zero of the instrument. The advantage of the short time constant, if I may use this expression, is fully proportional to its smallness, if it is not proportional to some higher power of its smallness.

I determined not to put up the apparatus in the doubtful atmosphere of London, and I am fortunate in having been able to fix it in my father's garden at Wing, in Rutland. The position is certainly good, the altitude is about 400 feet, the climate is as dry as in any part of England. The subsoil is oolitic limestone, containing a large quantity of iron, and very firm (the foundations of many of the old walls in the village are from 1 to 2 feet above the present level of the ground, and are perfectly secure). There is not a house or building

within 100 yards, and these are screened off by trees. The only objection is the rather long railway journey, which prevents isolated observations at odd times, and makes special observations of temporary phenomena very inconvenient.

The column is bolted down to a mass of about 2 tons of concrete, bedded upon the rock. The protecting house is made of wood and galvanised iron, and rests by four grooved wheels on rails made of gas-pipe, so that it can, when its own holding-down bolt is unscrewed, be pushed away so as to leave the telescope clear. The door lifts off and rests against two posts in one of the borders near. The figure will be sufficient to make every part, except minor details, perfectly clear. I think it will be best to describe the observations in the order in which they were made.

I began observations on the 6th September, 1888. The night was clear, but there was a gentle wind from the S.W., which produced an uncertainty in the position of the zero of a few millimetres. To keep off the wind I pulled the house over the telescope and looked east. Capella and Regulus gave no indication (11 20 P.M.), certainly not $\frac{1}{2}$ mm. There were several good negative observations. An earwig then began to climb up the delicate circuit of the radio-micrometer, and as it was windy I left, after first removing the circuit.

September 7th. To keep earwigs, of which there were an enormous number this year, and spiders from coming into the radio-micrometer, I placed in the diaphragm tube some cotton-wool which had been soaked in creosote. Thunderstorm in the day; at night wind north and cold. Dew on the telescope. Observed many stars up to 3 A.M., including Altair, Arcturus, α, β, and θ Orionis, and Capella. No deflection of as much as 1 mm. Wind prevented greater accuracy. About 3 A.M. some fleecy clouds passing produced strong effects of heat long before the star showed any diminution of brightness to the eye. A few leaves on the top of a distant tree produced an effect of about 60 mm. of heat.

September 11th. A new moon, just above the horizon (about 4°), produced, the instant the image of the limb met the disc, a rapid movement of about 30 mm., which gradually declined to about half when the terminator was reached, after which the deflection at once fell to nothing. There was no indication of heat from the dark part of the moon. The night being good, I was out till dawn, and tried all the bright stars in Pegasus, Andromeda, and Orion, as well as Aldebaran, Castor, Capella, and Saturn. No result from any of them, certainly not $\frac{1}{2}$ mm.

September 12th. At 5 30 P.M. the moon, first quarter, was low down in the south. Observations at 5 50 and at 5 54 in the daylight showed deflections at the limb of 125 and 120 mm., which, as before, became less towards the terminator, where they vanished.

September 13th. No effect from Jupiter, but he was badly placed near the S.W. horizon, and it was too windy for a satisfactory negative observation. Several observations of the moon showed the greatest heat to be close to the limb; the deflection for this part ranged from 175 to 200 mm.

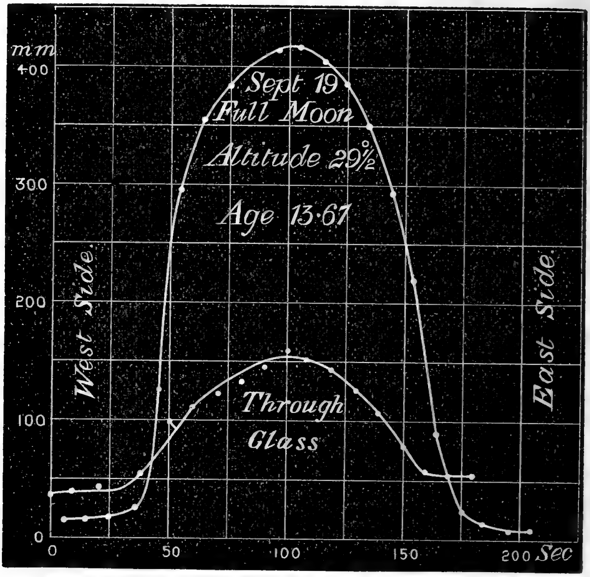
September 17th. Night clear and quiet; heavy dew. I now determined more accurately the variation of heat from point to point across the moon by arranging that it should transit centrally (or near a pole if desired) over the disc and taking readings every ten seconds. The first five curves in fig. 3 are the results of five consecutive transits

FIG. 3.

over the central part of the moon. The sixth curve was taken over the south end of the moon and the seventh curve over the north end. They were all taken when the moon was not far from the meridian. These are the first observations which clearly indicated a maximum of heat within the disc and not on the limb. This is evidently about the position at which the Sun is vertical, being approximately at right angles to the terminator. To save space and confusion, these curves are all drawn superposed but separated in time to a small extent.

September 19th. Full moon. The first curve, fig. 4, was taken when the heat of the moon was largely absorbed by a piece of clean window glass fixed across the mouth of the radio-micrometer. The second curve was taken immediately afterwards (9 5 P.M.) without glass. The heat transmitted by the glass, almost exactly 25 per cent., is somewhat different from the proportion, 17·3 per cent., which

FIG. 4.



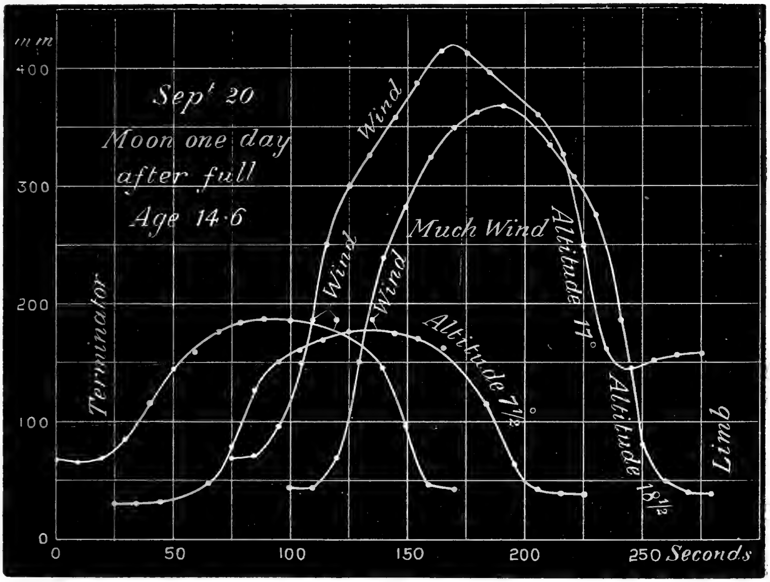
Lord Rosse obtained for the full moon, using the 3-foot telescope and a very superior thermo-junction of his own design.* The remarkable symmetry of the curve for the full moon at first rather puzzled me, as I expected that the side that had been baked by the sun for from 7 to 14 days would be hotter than the side that had only been lighted up for from 0 to 7 days. However, if we consider that soil of any kind is so bad a conductor that it would acquire its final surface temperature in perhaps an hour or less, that is, if not protected by an atmospheric blanket, the symmetry of temperature is nothing more than should be expected. Lord Rosse's experiments† on the heat of the eclipsed moon fully bear this out.

September 20th. Observed the moon on the horizon, deep red, limb ill defined, and cloud bands across, looking like the belts of Jupiter. A series of readings were taken, but not at exactly ten-second intervals. The deflection was greatest about the middle, where it reached 15 mm. Four minutes later (7 2 P.M.) the deflection in the middle amounted to 40 mm. The four curves, fig. 5, were taken later, as indicated by the increasing altitude. Though the later ones especially were much disturbed by wind, they well show the diminishing absorption by the atmosphere as the moon rose in the sky. Observations later

* 'Phil. Trans.,' vol. 163 (1873), p. 615.

† 'Roy. Dublin Soc. Trans.,' 1885, p. 321.

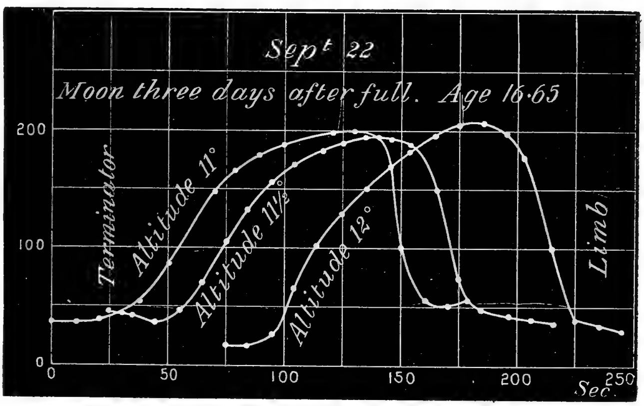
FIG. 5.



were prevented by light fleecy clouds which, as they flitted across the moon, so disturbed the deflections in the direction of cold (not as heat as when they pass over the sky or over stars) that the curves obtained later were a mass of teeth, like a comb or a saw, and ceased to have any value.

September 22nd. This was at first a perfect night, not a breath of

FIG. 6.



wind, but what there was was east. There was much dew, and by 9 P.M. the air had become filled with mist. Three observations on the moon (three days past full) were taken, but owing to the low altitude the actual amount of deflection was small. The regularity of the curves, which were the only ones taken that night, is an indication of the perfect quiet of the night. I examined also Vega, Altair, and α Cygni; there was certainly not a deflection of $\frac{1}{4}$ mm. The index often did not move $\frac{1}{10}$ mm. for several seconds. As this was a perfect night and the dark side of the moon was the advancing side, I made special observations to see if any heat could be obtained from the dark part on which the Sun had just ceased to shine, but not the slightest effect was observed until the terminator itself met the disc of the radio-micrometer.

I had at this time to go to London, and so no more observations were made until the early morning of October 31st, when I had returned to observe the moon a few days before conjunction. The night was windy after a wet day. At 3 30 A.M. the deflection on the limb was 25 mm. where it was greatest. As usual, it gradually decreased up to the terminator. At 6 30 A.M., when the moon was higher and better placed for a delicate test, I tried to observe the heat due to the earth-shine, which was then very marked. Owing to the wind I was unable to make a satisfactory negative observation, but it was certainly not 2 mm., and I believe not 1 mm. I have no record of the deflection on the bright part.

December 26th (5 30 P.M.). Venus very bright, but not at her best. An equal area in the moon would produce an effect easily observable, whereas a part of the moon large enough to produce the same light would still more produce a measurable deflection. There was not a deflection of $\frac{1}{2}$ mm. I had, however, another circuit in the instrument at this time which did not seem very sensitive.

April 19th, 1889 (7 30 P.M.). Made many observations on Venus, which had passed the position of greatest brilliancy. Sky clear, but the night was windy, which kept the index moving over a space of from 10 to 40 mm. Out of many transits observed, only two were not interrupted by a violent gust of wind. In each of these cases there was a deflection of from 2 to 3 mm. in the direction of heat, which may have been real, but I do not place any reliance upon it.

I was not satisfied with the delicacy of the apparatus and so made a new circuit, and suspended it by a fibre 1/5750 inch in diameter. With an exposed surface of about 4 square mm., illuminated by a candle flame at a distance of 60 inches, this gave a deflection of 60 mm.

As Mercury was unusually well placed in May, I took this circuit to Wing, hoping to obtain effects from the most promising of planets. Clouds, however, prevented Mercury from once being seen. Later on

I was able to observe Arcturus on the meridian in a clear sky, but I obtained no effect.

On March 25th, 1890, I tested the delicacy of this circuit with a 4-inch Leslie cube (black face) filled with boiling water at a distance of $81\frac{3}{4}$ inches. The deflection due to the exposed area of about 4 square mm. was 50 mm. A new circuit, made of better materials, gave a deflection of 180 mm. under the same conditions, and as this seemed a good circuit, I used it last Easter with freshly silvered mirrors.

April 2nd, 1890. A perfect night, except a slight but persistent east wind. The moon, not yet full, sent the index right off the scale at once. It was too windy to make satisfactory observations on stars.

April 3rd. Perfect night. Wind very slight, N.N.E. Very dry. Telescope tube and ether box covered with frost. Ether box so cold that no separate oxygen was necessary. I balanced a paraffin candle on a hedge at a distance of 93 yards, and found on moving the telescope on and off with the slow motion screw a very constant deflection of 55 mm. In this case the candle was so much out of focus that only about one-sixth of the rays fell on the disc, all the rest passing by. I could not alter the focus sufficiently, nor could I put the candle further away that night, but this was done the following day. About 1 30 A.M. Arcturus was on the meridian. There was not a deflection of $\frac{1}{4}$ mm. in any of a number of very satisfactory observations. Mars, low down in S.E., and Regulus and Saturn produced no effect either.

On Good Friday, April 4th, I prepared a clear view up to a mound 250 yards away, where a mill once stood, by cutting off the tops of the hedges that were in the line. On this mound I placed a stand carrying a beehive with a hole in the side. A candle placed in the beehive would send its rays into the distant telescope without any intermediate obstruction. A light paper shutter inside the beehive was arranged so that it could be pulled to one side by a long piece of cotton, and be allowed to fall back again so as to obstruct the radiation. The exact distance from the candle to the mirror (obtained by chaining) was 250·7 yards. During the day I moved the radio-micrometer on its geometrical slide until the disc and the image of the beehive were in the same plane. The image of the candle-flame, which was about $\frac{1}{4}$ mm. long, was then focussed upon the disc, and all the rays which entered the telescope from any part of the flame were received upon the disc.

About 8 30 P.M., when it was fairly quiet, there being a slight east wind, my niece, L. Wintle, kindly stationed herself on the mound out of sight of the telescope, and at a signal either pulled or let go the cotton. The telescope was pointed towards the candle the whole

time. It was not moved, nor was anything moved, except the shutter in the beehive. At each movement of the shutter the image of the light started off violently, and came to rest in about five seconds, showing a deflection of 38 mm. This was repeated about twenty times. This is an absolutely trustworthy measure of the sensibility of the whole apparatus, including imperfect reflection by the silver and every factor, except the absorption by the air. This, as the night was dry and clear, must have been very small. It shows if $\frac{1}{4}$ mm. is taken as the smallest deflection which can be observed with certainty, that a candle-flame at 250·7 yards produces an effect which is 152 times as great as the least which can be certainly observed; or that, if the candle-flame had been taken $\sqrt{152}$ or 12·3 times as far away, that is, 3084 yards, or 1·71 miles, it would have sent into the telescope sufficient heat to be observed, and therefore more than Arcturus.

After these measures were made, the stand, &c., were removed, and my niece stood with her face exactly in the line of the telescope. The image of her face must have been entirely within the disc. When she moved on one side the clear sky was in view instead. The deflection, observed many times, was 48 mm. This large effect was no doubt chiefly due to the screening of the sky, but I mention the experiment to show that the mirrors reflect dark heat satisfactorily.

Later on in the evening, at about 10 P.M., I turned the telescope on to the moon, now about full. The index was, of course, driven right off the scale, but with a 6-inch stop over the mouth, the deflection, due to the centre of the moon's disc, was 150 mm. With a 12-inch stop the index again went off the scale. Now the area of the mirror, which is 15·625 inches in diameter, is 192 square inches, the area of the 6-inch stop is 28·25 square inches, and the area obstructed by the flat is 4·67 square inches. The effective area of the mirror is therefore 187·3 square inches, and of the 6-inch aperture 23·58 square inches, and the ratio of these numbers, 7·95, is the number by which the deflection with the 6-inch stop must be multiplied in order to obtain the deflection that would be obtained from the whole mirror if all could still go in proportion. The centre of the full moon, then, would give, on this supposition, a deflection of 1192·5 mm., or about 4800 times as great an effect as could be observed with certainty. Since the disc is in the particular instance about one-sixth of the diameter of the image of the moon, it is clear that it covers about 1/36 or receives, in round numbers, 1/30 of the total heat, if it is at the centre. It is evident, then, that an amount of heat equal to about 1/150,000 of that sent by the full moon would produce an effect which could be observed with certainty, and Arcturus does not send this.

Dr. Huggins did not obtain any effect from the moon, but, as he used an object glass which would absorb nearly all the moon's heat,

or, at any rate, a large proportion, and as it had a smaller aperture and a greater focal length than the mirror which he lent me, so that the image of the moon was larger, and on this account also had a smaller heating power, it is not surprising that he did not obtain a large effect from the moon; but that he obtained none is, in the face of these measures, strong evidence that the deflections observed, and which he attributed to the stars, were spurious.

Mr. Stone did not observe the moon at all, but he concluded from his observations that Arcturus was in heating power equivalent to a 3-inch cube of boiling water at 400 yards. A direct comparison of the candle that I used with a 4-inch cube of boiling water gave as the ratio of the heating powers on a radio-micrometer, of which the temperature was 15° C., a figure varying slightly with the state of the flame, but generally slightly over one half. Had the comparison been made with the radio-micrometer at 0° C., as it was when the observations on Arcturus were made, a slightly lower number would, of course, have been obtained. As the face of a 3-inch cube is slightly over one-half the face of a 4-inch cube, the candle and the 3-inch cube may be taken for the purpose of comparison as sensibly equal. Now, the direct and conclusive experiment already described has shown that Arcturus on the meridian is not equal to a candle, and therefore to a 3-inch cube at 3084 yards, so that Arcturus must have a heating power nearly sixty times less than that given by Mr. Stone. I cannot clearly follow the whole of Mr. Stone's reasoning, more especially that part which relates to the rise of temperature due to the star. By the use of thermometers in contact with the face of his pairs when subject to a much stronger radiation, Mr. Stone determined the amount by which the face must be raised in temperature to produce a deflection of one division, and he concluded from the deflection observed that the face was raised through 1/90° C. Now, on comparing his arrangement with mine, it is evident that, as I have a larger aperture, I have more heat to begin with; as I have no glass for the rays to pass through, I am free from the large absorption of glass for heat; and, finally, as my thermo-electric bars have a sectional area about one-twentieth of that of the bars used by Mr. Stone, the heat for a given difference of temperature would have been conducted away at about one-twentieth of the rate; or, as this is the chief cause of the cooling of the hot junction, for a given rate of radiation my junction would have been heated to nearly twenty times the amount to which Mr. Stone's would be heated. Taking all these things into consideration—if Mr. Stone's figures are correct—my junction should have been warmed through about one-third or one-fourth of a degree Centigrade. Now, I have already shown*

* Cantor Lectures, 1889.

that the radio-micrometer will respond to a temperature rise of less than one-millionth of a degree. Even if my figure of one-millionth is not absolutely correct—and I believe that the figure should be still smaller—the discrepancy is so enormous as to require explanation. Knowing how completely every thermopile that I have examined fails to approach in delicacy the radio-micrometer, I cannot quite understand how Mr. Stone was able to observe deflections which correspond to a rate of radiation which is only a few times as great as the least that I can observe. As an instance of the delicacy of the radio-micrometer, I may state that, compared with the usual form of thermopile, made by Messrs. Elliot Brothers, connected with a low resistance reflecting galvanometer by the same makers, it is fully one thousand times as sensitive, besides being far more stable or insensitive to disturbing causes.

I think my observations go to show that the heat of Arcturus has not as yet been observed (unless the refracting telescope possesses some mysterious power which the reflector does not); and the same conclusion may be drawn with respect to the other stars. I have by no means reached what I believe to be a practical limit to the delicacy of the radio-micrometer, and it is possible that with a more delicate instrument, or with a larger telescope—and Mr. Common has promised to allow me to use for the purpose his 5-foot reflector—I may yet be able to observe some definite and real effect; but that I should have so far failed is not surprising, in view of the following argument. When anything is heated and gives out light an increment of temperature causes a relatively greater increment of light; so that, light for light, less heat is radiated by the hotter body. Thus, among stars that are equally bright, the coolest will send the most heat. Now, a candle-flame is, in all probability, far cooler than any white or yellow star; and so if a candle-flame is removed until it appears of the same brightness as some star—for instance, Arcturus—then it should radiate far more heat. Now, I do not know at what distance a candle would appear as bright as Arcturus, but supposing that it is as much as 1·71 miles, then, since at this distance I can only just detect the heat of the candle, it is evident that I should not be able to detect the heat of Arcturus.

With regard to the observations on the moon, they are, I know, exceedingly fragmentary and incomplete. They were not, however, undertaken for their own sake, though they may have some value, but in order to test the apparatus and to serve as a standard by which other observations might be compared with mine. For work on the moon the apparatus is not exactly the most convenient, though it does well. For systematic work I should employ the most delicate circuit I could make with a very small sensitive surface and a quick period, and then, to obtain curves of heat which should give the deflections

from moment to moment and not only at ten seconds intervals, I should record the movement of the index photographically. It would also be an advantage to have the telescope under cover, owing to the annoyance caused by wind. I did not adopt this plan, as I did not wish the apparatus to be influenced by any unnecessary source of heat. Now that I have found how insensitive it is to everything in the nature of radiation, except that which is focussed by the telescope upon the disc, I know that this precaution was unnecessary as well as inconvenient. The apparatus, as designed, is not suitable for spectroscopic examination of the moon's heat, and so I have not attempted to repeat any of Langley's observations. I may state that, though neither the properties of the circuit employed nor the ten seconds readings were suitable for detecting local variations of the moon's heat, due to bright or dark spots, yet I did expect to find indications of local differences. None, however, were apparent, but I can hardly imagine that with a much smaller disc, a more rapid period, and a photographic record local variations could not be determined.

As my observations on the moon's heat are so small in number and were made at such irregular intervals, I have not applied Lord Rosse's correction* for its absorption by the air.

The curves of the moon's heat require a slight correction for the lag of the apparatus, which I have not attempted to make and which it would be useless to make unless the curves were drawn by a more perfect method. The correction could be reduced to a great extent by causing the telescope or a siderostat to so move as not quite to keep pace with the earth's rotation, so that a greater time would be occupied by a transit; but it would be unsafe to prolong this time to a great extent, for, besides the greater uncertainty in the position of the zero, a slight change would be produced by the varying altitude of the part of the sky under observation, and this would change the atmospheric absorption. As the telescope is moved from the horizon to the zenith the index moves over the scale to an extent which is very variable, but in the direction of cold.

I should have stated that very thin clear mica absorbs a large proportion of the moon's heat. On the night that I tried this I was unable to measure the proportion, as while with the mica I obtained a deflection (not recorded in my notes) of roughly 200 mm., without the mica the index went off the scale and I had no stop to limit the radiation.

Anticipating trouble owing to the varying state of the sky, such as was found by Mr. Stone, I devised a form of radio-micrometer circuit which should be differential. Thus, calling the alloys used A and B,

* 'Phil. Trans.,' 1873, p. 598.

two A bars are soldered to the lower ends of the copper wire arch, two B bars are soldered to these and joined below. The A, B junctions are then exposed in the radio-micrometer side by side, and so are unaffected by general atmospheric influences, while as a star passes from one to the other the effect of the star will be reversed. On a quiet night the single junction works so well that I have not at present tried the differential form.

It is my intention to make observations from time to time when I am able, but these, owing to my duties in London, can only be made at uncertain intervals.

[*Note.*—At the time that this paper was sent in I did not realise that the larger figure that I obtained for the percentage transmission of the moon's heat through glass, viz., 25, instead of about 17, as found by Lord Rosse, far from being of the nature of a discrepancy, is in reality a difference which should be expected. Lord Rosse made observations on the total heat only; he did not localise parts of the moon, as I have done. He found a greater proportionate transmission at the full moon than at the quarters, viz., 17 at the full and 8 at the quarters. Now, taking elements in the moon, I cannot think that this proportion can to any appreciable extent depend on the altitude of the earth at the element, but it should depend on the altitude of the sun there, for upon this the heat received by the element depends. Now, in taking the full moon as a whole, there are elements with the sun at all altitudes from 0° to 90°; but those parts with the sun at the zenith are most favourably situated to produce their effect on the earth, and thus the observed effect, which is the result of all the parts, should show a percentage transmission between that due to a zenith sun and a sun near the horizon, which should also be the case with the moon at the quarters or between the quarters and the full; but there is this difference, as the moon approaches the quarters the effect of the parts on which the sun is shining vertically becomes proportionately less and less, and so while with a full moon the percentage transmission should approach that due to an element with a vertical sun, with a half moon the percentage should be more nearly that due to an element with a rising or setting sun.

Now my method of observation of the moon at once gives the means of finding the percentage transmission due to an element with the sun at any altitude. The curve, fig. 4, shows that the centre of the full moon does send a larger proportion through glass than either limb, and, as it should do, a greater proportion than the integral result found by Lord Rosse. A single transit of the full moon should, therefore, give data from which the integral result of the moon in any phase could be calculated, and corresponding observa-

tions upon the moon in other phases would show experimentally whether the percentage transmission by glass of the heat from an element of the moon does depend on the altitude of the sun only, or whether the altitude of the earth also has any influence, as, indeed, is suggested by Lord Rosse in a note to his Bakerian Lecture ('Phil. Trans.,' vol. 163, p. 626). It is unfortunate that I have not at present made any other observations on the transmission by glass of the moon's heat.—April 21, 1890.]

IV. "Observations on the Secretion of Bile in a case of Biliary Fistula." By A. W. MAYO ROBSON, F.R.C.S., Hon. Surgeon, Leeds General Infirmary, Lecturer on Practical Surgery at the Yorkshire College, and Examiner in the Victoria University. Communicated by Dr. CLIFFORD ALLBUTT, F.R.S. Received April 3, 1890.*

There are few physiological questions on which so much doubt and disagreement prevail as on that of the secretion and uses of bile, this being especially marked when we come to compare the apparently contradictory observations of various experimenters relating to the action of drugs on the biliary secretion.

As the well known experiments of Dr. Rutherford and Messrs. Prévost and Binet were conducted on the lower animals, it may possibly account for the differences between their observations and those recorded in this paper. From the rarity of cases of biliary fistula in healthy human subjects, the opportunity has rarely occurred for a careful analysis of fresh bile in sufficient quantity, or for a complete analysis of the *whole* twenty-four hours' secretion; and in all previous analyses no notice has been taken of the gall-bladder secretion.

In the following cases, the fistulæ remained open for long periods after the initial operations, the total flow of bile or gall-bladder secretion was carefully collected and accurately measured at different times and for many consecutive hours at a time, and the general good health of the patients was maintained throughout.

Method of Collecting.

The fluid was caught in a light glass flask, into the mouth of which it was guided by means of a celluloid cannula, a substance chosen after several trials with metal ones, on account of its lightness and non-irritating qualities.

* This paper is a revision of that read on January 16, under the title "Observations regarding the Secretion and Uses of Bile" (see p. 129, *supra*).

Case I.—*Biliary Fistula.*

Mrs. V. B., aged forty-two, was operated on in January, 1888, for the relief of obstruction in the common bile duct. The incision was made over the gall-bladder, which was brought to the surface, relieved of its contents, and opened, the margin being sutured to the edge of the abdominal wound and drained. The patient made a good recovery from the operation; but a biliary fistula persisted, through which was discharged the whole of the bile for fifteen months. In order to ascertain that the whole of the bile secreted escaped through the fistula, and that none entered the bowel, repeated analyses of the urine and fæces were made, but no evidence of the presence of bile was obtained at any time. The fistula was ultimately closed by stitching the gall-bladder to the bowel, and making a communication between them, thus enabling the bile to reach the intestine by another channel. A detailed description of the case will be found in the 'Transactions of the Royal Medical and Chirurgical Society' for 1889.

Influence of Biliary Fistula on Digestion and Nutrition.—During the fifteen months that the fistula was open, the patient's digestion seemed to be unimpaired. The appetite generally was good; there was a craving for acids, such as lemons and pickles, and a dislike to sweet foods, to meat, and to fat. Much fatty matter in her food had a marked effect, producing a sickly feeling, with loss of appetite, and rather more fat than normal was then noticed in the fæces. Her bowels were quite regular without the use of aperients, and the odour of the fæces did not differ from that of healthy motion. Menstruation never occurred during the time the fistula was patent, but, as soon as the bile was again turned into the intestine, the menstrual function became regular and normal.

Case II.—*Fistula of Gall-bladder not Biliary.*

Mrs. A., aged thirty-two, was operated on in June, 1884, for distended gall-bladder due to gall-stones, with stricture of the cystic duct; the patient made a good recovery from the operation, but a fistula of the gall-bladder persisted. From this opening a constant flow of a clear and somewhat viscid fluid persisted, which was held to be the normal secretion of the gall-bladder, as there was complete obstruction of the cystic duct, and as no bile constituents were found in the fluid at the time of the operation or subsequently.

Analyses of the fluid from this patient were made in October, 1885, and in April, 1887, by Professor de Burgh Birch, of the Yorkshire College (see 'Journal of Physiology,' vol. 8, No. 6), and in March, 1889, by Mr. Fairley, F.C.S., F.R.S.E. In the appended tables will

be found Mr. Fairley's analysis of the secretion for twenty-four hours.

The alleged *Diastatic Action* of bile may possibly be due to the admixture of the secretion from the gall-bladder, or from the mucous glands in the large bile ducts. In the gall-bladder fluid from Case II, Professor Birch found a diastatic ferment, concerning which he reported :—"The secretion cannot be regarded as having any important part to play in digestion, the small diastatic action it possesses on starch being shared by many fluids in the economy upon which it does not confer any special digestive value." ('Journal of Physiology,' vol. 8, No. 6.)

Antiseptic Action.—In Case I, the value of bile as an antiseptic in the intestine could be tested only by the character of the fæces, which, over a period of fifteen months during which no bile entered the bowel, did not by odour or aspect indicate any irregular fermentative process. In Case II, the constant clean appearance of the edge of the fistula suggested to me the idea that it might be due to the antiseptic quality of the gall-bladder fluid ; and the observation, that when collecting the fluid for experimental purposes I could leave the flasks exposed to the air for several days without any apparent change, suggested the same conclusion. Professor Birch, from numerous cultivation experiments, came to the conclusion that its antiseptic properties were slight, the want of change being rather due to the poverty of the fluid in nourishing materials. ('Journal of Physiology,' vol. 8.)

Aperient Action.—In Case I, the bile did not seem to be at all necessary as an intestinal stimulant, for the bowels were quite regular during the whole of the time that no bile was entering the intestines.

Alleged Action of Bile in Promoting Absorption.—In Case I, fat could apparently be digested in quantities sufficient not only to maintain normal nutrition and good health, but to lead to an increase in weight. If taken too freely it seemed to create disturbances of digestion, and to be passed in rather larger quantities than usual in the fæces, as ascertained by careful observation and by separation by means of ether.

Diet.—Details of the daily diet are given in the tables, and may be grouped as follows :—

I. Oct. 24th—27th. Light diet. Broth, bread, egg, tea, milk, pudding.

II. Oct. 29th—Nov. 4th. Chicken diet. Ditto with chicken.

III. Nov. 5th—8th. Potato diet. Ditto with potato.

IV. Nov. 12th. Meat diet. Meat, bread, milk, tea.

Flow of Bile.

The Tables appended show the dates and hours of collection lasting over a period of eight months; the nature and quantity of the diet; and the amount of bile excreted. The Charts also show the dates of administering certain medicines, and their effect or absence of effect on the biliary secretion.

In the drawing up of the charts and tables I have been greatly assisted by my friend Mr. C. W. Biden.

Daily Quantity of Bile Flow.—In Case I, the quantity of bile collected in twenty-four hours on various dates in October, November, and December of 1888, and January, February, March, and April of 1889, varied from 39·53 oz. to 25·86 oz., and averaged 29·98 or nearly 30 oz. In Case II the gall-bladder fluid measured 2·53 oz. in twenty-four hours.

Subtracting this amount from the twenty-four hours' discharge in Case I, we get the average daily flow of bile as 27½ oz.

Diurnal Variation in Flow.—The Tables and Charts show distinctly that more bile is invariably excreted during the day than at night; the difference at times being as much as 5 oz., at others not more than 3 dr.

In the tables and charts which show an hourly collection for over twenty-four hours, it is clearly seen that the excretion of bile is continuous night and day. These measurements were carefully and regularly made by the sisters in charge of the ward, under the supervision of the resident surgical officer, Mr. H. Littlewood, F.R.C.S., and my house surgeons, Mr. B. Moynihan, M.B., F.R.C.S., and Mr. F. Hudson, M.R.C.S., to whom I am much indebted for the great pains they took over so long a period.

The daily quantity does not correspond with the observations of von Wittich and Westphalen, who reported a collection of one pint in the twenty-four hours, with but small variations during ten days.

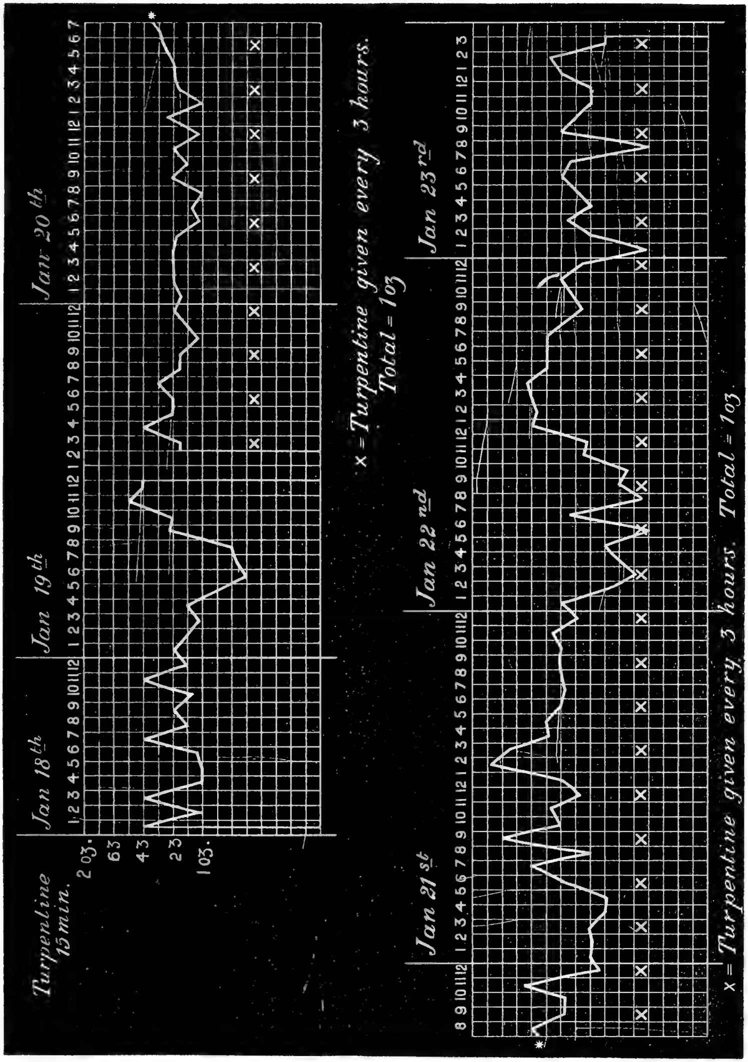
More solids are contained in the bile by night than by day, as is shown by the analysis of the specimens which were examined by Mr. Fairley (see appended Tables).

The quantity of bile discharged is apparently not much influenced by the ingestion of food. The reception of food into the stomach is generally contemporaneous with a marked decline in the flow of bile, lasting for about two hours. The colour of the fresh bile was always green. The violet odour of turpentine was perceived in the bile soon after its administration.

The Effect of Drugs on the Bile Flow.

The observations made on the effect or non-effect of certain drugs on the biliary secretion show results which are at variance

CHART 1.



with the usually accepted views of the action of medicines on the liver.

Calomel.—On Nov. 7th, 1888, 5 gr. of calomel were administered at 7 P.M.; a slight aperient effect followed the next morning, but, on comparing the amount of bile excreted before and after, it was found that for ten hours before the administration of the calomel,

CHART 2.

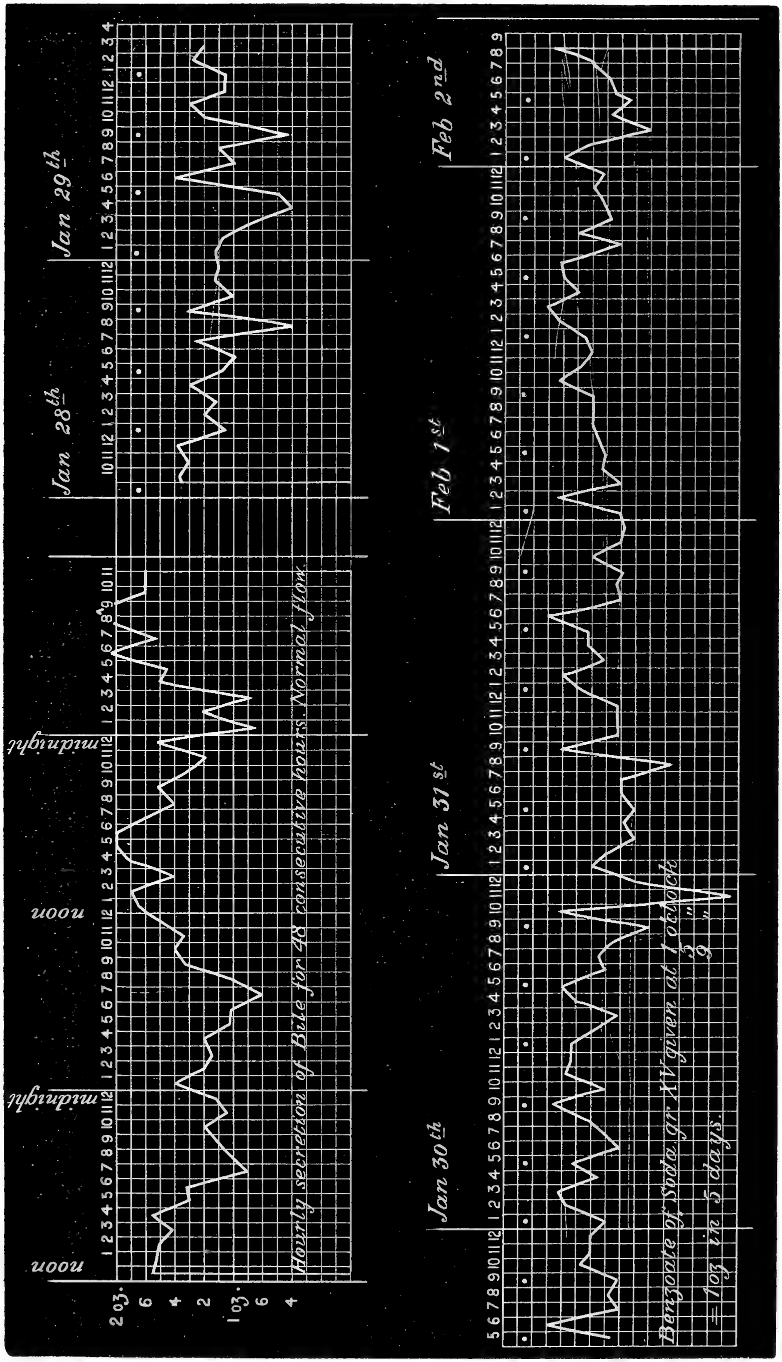
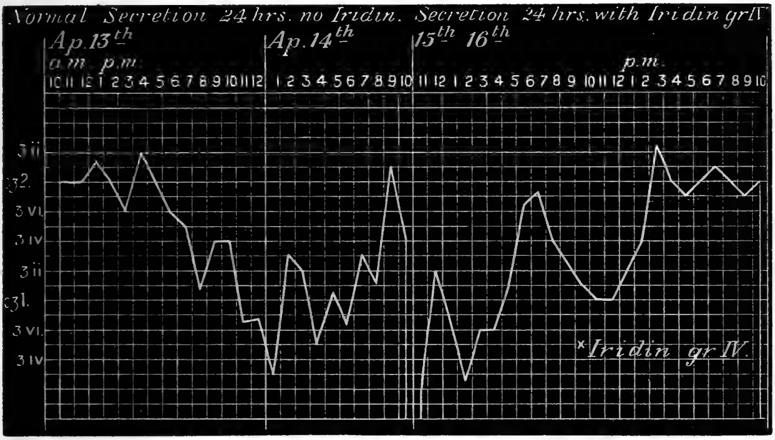

Jan 28th
Jan 29th
noon
midnight
noon
midnight
2 oz.
1 oz.
Hourly secretion of Bile for 48 consecutive hours. Normal flow.
Jan 30th
Jan 31st
Feb 1st
Feb 2nd
Benzoate of Soda gr XV given at 1 o'clock
= 1oz in 5 days.

CHART 3.



12 oz. 6 dr. 20 min. of bile were excreted, and that for ten hours subsequent to the administration 10 oz. 4 dr. 30 min. were excreted, *i.e.*, 2 oz. 1 dr. 50 min. less.

Euonymin.—On Nov. 17th 4 gr. of euonymin were given at 11·30 A.M.; for the four hours preceding the administration, 5 oz. 4 dr. 9 min., and during the four hours subsequent to its administration, 5 oz. 1 dr. 8 min., were excreted, *i.e.*, 3 dr. less. This dose was repeated on several occasions with similar results.

Rhubarb.—On Nov. 13th, at 11 A.M., $\frac{1}{2}$ oz. of tincture of rhubarb was administered; during the preceding six hours 7 oz. 3 dr. 23 min. of bile were excreted, and during the six hours subsequent to the administration of the drug, 7 oz. 4 dr. 19 min. were excreted, that is 56 mins. more, in the subsequent than in the preceding six hours. But on comparing the corresponding period of the previous day, when no rhubarb was given, we find that 8 oz. 6 dr. 10 min., or $1\frac{1}{4}$ oz. more, were excreted. Therefore no increased flow of bile can be put down to the action of the rhubarb.

On Nov. 15th, 1 oz. of tincture of rhubarb was given. The figures as seen in the tables again show a diminution compared with the previous day.

Podophyllin was given on one occasion, and no cholagogue effect was noticed.

Carbonate of Soda.—Soda water, aërated, was given, and produced an increased flow. Its ingestion was followed in two hours by a maintained increased flow not succeeded by a marked diminution.

Iridin.—On April 16th, 4 gr. of iridin was followed by a good afternoon rise in the bile flow, but two days later there was a much higher afternoon rise when no drug had been given. On April 19th,

4 gr. of iridin gave an effect not so pronounced, the increased flow being intermittent. Apparently, the action of iridin is to increase the flow temporarily, without augmenting the total quantity in twenty-four hours.

Turpentine.—Messrs. Prévost and Binet state that turpentine and its derivatives promote a notable increase in the excretion. In order to test this, a turpentine capsule containing 15 min. of the oil of turpentine was given every four hours night and day.

On Jan. 18th, no drug given. 27 oz. 6 dr. 35 min. were excreted in twenty-four hours. On Jan. 19th and 20th, during the administration of turpentine capsules, 28 oz. 5 dr. 41 min. were excreted, that is, an increase of 7 dr. During the following twenty-four hours, the capsules being continued, 30 oz. 2 dr. 10 min. were excreted.

During the third period of twenty-four hours with the capsules 26 oz. 57 min. were excreted; and during the fourth twenty-four hours 27 oz. 45 min.

Therefore, although an increase was apparent on the second day, the daily amount of bile discharged in the twenty-four hours was not so much as on many days when no turpentine was being given, as, for instance, on Oct. 27th and 29th, when it was over 30 oz.

Benzoate of Soda.—Messrs. Prévost and Binet state that the administration of benzoate of soda to dogs increased the amount of bile to two or three times the normal. This I do not find to be the result in Case I, as the table and charts appended will show, where no positive increase is seen.

Conclusions.

First.—The bile is probably chiefly excrementitious, and, like the urine, is constantly being formed and cast out.

Secondly.—Though the bile probably assists in the absorption of fats, its presence in the intestine is not necessary for the digestion of such an amount of fat as is capable of supporting life and keeping up nutrition.

Thirdly.—Increase in body weight and good health are quite compatible with the entire absence of bile from the intestines.

Fourthly.—The antiseptic properties of the bile are unimportant.

Fifthly.—Whatever little antiseptic quality bile may have is probably derived from its admixture with the gall-bladder fluid.

Sixthly.—The supposed stimulating effect of the bile on the intestinal walls is not necessary for a regular action of the bowels.

Seventhly.—The quantity of bile excreted in the twenty-four hours, during health in a person of average weight, may vary between 39 oz. 4 dr. and 25 oz. 6 dr., with an average of 30 oz., less the $2\frac{1}{2}$ oz. of fluid secreted by the gall-bladder.

Eighthly.—More bile is excreted during the day than at night, the excess varying between 5 oz. and 3 dr.

Ninthly.—The excretion of bile seems to go on constantly and with great regularity.

Tenthly.—The excretion is apparently not materially influenced by diet.

Eleventhly.—The pigment of fresh human bile is biliverdin.

Twelfthly.—The supposed cholagogues investigated seem to rather diminish than increase the amount of bile excreted.

Mr. Fairley's Analysis.

Analysis of bile drawn from biliary fistula (Mrs. V. B.), collected April 13th, 10 A.M. to 10 P.M., and April 13th—14th, 10 P.M. to 10 A.M., 1889.

Columns I, II, III refer to the whole bile and gall-bladder fluid: Column I, first twelve hours; Column II, second 12 hours; and Column III, the whole fluid collected during twenty-four hours. Column IV gives the composition of the bile calculated without the gall-bladder fluid.

	I. 12 hours' bile, 10 A.M. to 10 P.M. April 13.	II. 12 hours' bile, 10 P.M. to 10 A.M. April 13—14.	III. 24 hours' bile, April 13—14.	IV. 24 hours' bile, corrected for gall-bladder fluid.
Quantity	570 c.c.	370 c.c.	940 c.c.	868 c.c.
Specific gravity	1·0085	1·0090	1·0087	1·0086
Reaction	Alkaline.			

The bile contains in 1000 parts:—

	I.	II.	III.	IV.
Water	982·10	981·79	981·98	981·76
Total solids	17·90	18·21	18·02	18·24
	1000·00	1000·00	1000·00	1000·00

The solid matter of the bile contains:—

	I.	II.	III.	IV.
Cholesterin	0·44	0·45	0·45	0·45
Fatty matter (free)	0·11	0·12	0·12	0 12
Fat combined (chiefly sodium stearate)	0·90	1·08	0·97	0·97
Sodium glycocholate	7·45	7·60	7·51	7·51
Sulphur equal to sodium taurocholate	0 087	0·094	0·09	0·09
Organic substances precipitated by alcohol, chiefly				

	I. 12 hours' bile, 10 A.M. to 10 P.M. April 13.	II. 12 hours' bile, 10 P.M. to 10 A.M. April 13—14.	III. 24 hours' bile, April 13—14.	IV. 24 hours' bile, corrected for gall-bladder fluid.
mucus and epithelium	1·31	1·29	1·30	0·85
Chlorides equal to sodium chloride	5·08	4·91	5·01	4·95
Carbonates and phosphates of sodium, potassium, lime, magnesia, and iron	2·52	2·66	2·57	2·54
Copper	..	minute trace	..	trace
Silica	..	trace	..	trace
Sulphates, Urea, Sugar	..	none	..	none

The solid matter of the bile gave on ignition :—

Ash per 1000 parts	8·15	8·68	8·36	8·34

The above analysis of the bile was confirmed by a further quantitative analysis of the bile taken five days later.

The average quantity of bile as ascertained by observations extending over eight months was 30 ounces (very nearly 862 c.c.) during twenty-four hours.

Analysis of Fluid from the Gall-bladder (collected during 24 hours. Mrs. A.). Received April 29, 1889.

Quantity	72 c.c.
Specific gravity	1·0095
Reaction	Alkaline.

The fluid contains in 1000 parts :—

Water	984·64
Total solids*	15·36

The solid matter contains :—

Organic matter, chiefly mucin with trace of albumen	6·72
Chlorides equal to sodium chloride	5·73
Sodium carbonate	2·20
Other salts, containing phosphates, potassium salts, &c.	0·71

* The solid matter was carefully dried until its weight was constant, and on ignition gave 8·64 parts of ash.

Schedule showing Amount of Variation in Flow over a Period due to the Drug.

Drug.	Duration of observing period.	Difference in flow from: The preceding period same day.	Difference in flow from: Contemporaneous period previous day.	Actual flow during period stated with drug.	Date.
	hrs.	oz. dr. min.	oz. dr. min.	oz. dr. min.	
Calomel	10	−2 1 50	+1 0 20	10 4 30	Nov. 7
Rhubarb	6	+0 0 56	−1 1 51	7 4 19	„ 13
„	6	+0 4 51	−2 6 46	7 3 24	„ 15
Euonymin	4	−0 3 1	−0 2 12	5 1 8	„ 17
Turpentine	24	..	+0 7 6	28 5 41	Jan. 19
„	„	..	+1 4 29	30 2 10	„ 20
„	„	..	−4 1 13	26 0 57	„ 21
„	„	..	+0 7 48	27 0 45	„ 22
Soda benzoate	„	..	−1 4 30	26 1 5	„ 28
„	„	..	+3 5 10	29 6 15	„ 29
„	„	..	−3 3 10	26 3 5	„ 30
„	„	..	+2 1 15	28 4 20	„ 31
„	„	..	+2 0 25	30 4 45	Feb. 1

Minus = decrease of "so much," possibly due to drug.
Plus = increase of "so much," possibly due to drug.

Mrs. V. B. Age 42. Daily Excretion of Bile.

		oz.	dr.	min.
Oct. 24—				
12—1 P.M.	Fish, 6 oz.; pudding	1	4	59
1—2	.. .. ..	1	4	30
2—3	.. .. ..	1	1	40
3—4	.. .. ..	1	1	40
4—5	Tea, 14 oz.; bread, 5½ oz.; egg, 1......	1	1	0
5—6	.. .. ..		7	0
6—7	.. .. ..	1	3	0
7—8	Milk, 1 pint	1	2	0
8—9	.. .. ..	1	2	46
9—10	.. .. ..	1	2	0
10 P.M.—7 A.M.	Milk, 1 pint	6	5	0
7—8	Tea, 16 oz.; bread, 5½ oz.	1	2	30
8—9	.. .. ..	1	4	0
9—10	.. .. ..	1	2	40
10—11	Beef tea, 1 pint	1	3	30
11—12 noon	.. .. ..	1	2	0

	oz.	dr.	min.
Total quantity excreted in 24 hours	26	2	15
From 10 P.M. to 10 A.M.	10	6	10
,, 10 A.M. to 10 P.M.	15	4	5

		oz.	dr.	min.
Oct. 25—				
10—11	Beef tea, 1 pint	1	3	30
11—12 noon	.. .. ..	1	2	0
12—1	.. .. ..	1	1	45
1—2	.. .. ..	1	1	40
2—3	.. .. ..	1	2	0
3—4	Tea, 10 oz.; bread, 6 oz.; egg, 1	1	0	30
4—5	.. .. ..	1	1	0
5—6	.. .. ..	1	3	0
6—7	Chicken broth, 12 oz.	1	4	0
7—8	.. .. ..	1	1	0
8—9	.. .. ..	1	3	0
9—10	Milk, 1 pint		7	0
10 –5 A.M.	.. .. ..	7	5	0
5—6	.. .. ..	1	1	30
6—7	.. .. ..	1	0	30
7—8	Tea, 10 oz.; bread, 4½ oz.	1	2	0
8—9	.. .. ..	1	3	0
9—10	.. .. ..	1	4	0

	oz.	dr.	min.
10 A.M. to 10 P.M.	14	6	25
10 P.M. to 10 A.M.	14	0	0
	28	6	25

Mrs. V. B. Age 42. Daily Excretion of Bile.

		oz.	dr.	min.
Oct. 26—				
10—11	.. .. ..	1	2	25
11—12 noon	.. .. ..	1	4	0
12—1	Broth, 18 oz.; pudding, 11 oz.	1	0	37
1—2	Bread, 1 oz.	1	3	30
2—3	.. .. ..	1	2	0
3—4	.. .. ..	1	4	0
4—5	Tea, 16 oz.; bread, 5 oz.; egg, 1.	1	0	35
5—6	.. .. ..	1	4	0
6—7	.. .. ..	1	0	40
7—8	Milk, 1 pint		6	50
8—9	.. .. ..	1	2	15
9—10	.. .. ..	1	2	0
10—5 A.M.	Milk, 1 pint	7	6	0
5—6	.. .. ..	1	2	0
6—7	Tea, 10 oz.; bread, $4\frac{1}{2}$ oz.	1	0	0
7—8	.. .. ..	1	0	0
8—9	.. .. ..	1	5	0
9—10	.. .. ..	1	4	35

	oz.	dr.	min.
10 A.M. to 10 P.M.	15	0	52
10 P.M. to 10 A.M.	14	1	35
	29	2	27

		oz.	dr.	min.
Oct. 27—				
10—11	.. .. ..	1	4	35
11—12 noon	Broth, 17 oz.; pudding, $7\frac{1}{2}$ oz.	1	4	0
12—1	.. .. ..	1	0	0
1—2	.. .. ..	1	0	0
2—3	.. .. ..	1	2	10
3—4	.. .. ..	1	3	33
4—5	Tea, 17 oz.; bread, 5 oz.; egg, 1	1	2	0
5—6	.. .. ..	1	4	0
6—7	.. .. ..	1	2	0
7—8	Milk, 19 oz.	1	2	50
8—9	.. .. ..	1	0	0
9—10	.. .. ..	1	2	0
10—5 A.M.	.. .. ..	7	6	0
5—6	.. .. ..	1	0	0
6—7	.. .. ..	1	2	55
7—8	Tea, 16 oz.; bread, $5\frac{1}{2}$ oz.; milk, 10 oz.	1	4	0
8—9	.. .. ..	1	4	0
9—10	.. .. ..	1	5	0
10—11	.. .. ..	1	2	50
11—12	.. .. ..	1	4	45

	oz.	dr.	min.
10 A.M. to 10 P.M.	15	3	8
10 P.M. to 10 A.M.	14	5	55
	30	1	3

Mrs. V. B. Age 42. Daily Excretion of Bile.

Oct. 29—		oz.	dr.	min.
7—8	Tea, 10 oz.	1	2	45
8—9	Bread, 5 oz.	2	0	0
9—10	Milk, 9 oz.	1	5	40
10—11	.. .. ..	1	3	0
11—12 noon	.. .. ..	1	1	0
12—1	Chicken, 7 oz.; pudding, 6 oz.	1	5	11
1—2	.. .. ..	1	4	0
2—3	.. .. ..	1	4	0
3—4	.. .. ..	1	3	0
4—5	Tea, 15 oz.; bread, 5½ oz.; egg, 1	1	2	50
5—6	.. .. ..	1	2	0
6—7	Milk, 12 oz.	1	0	57
7—8	.. .. ..	1	3	30
8—9	.. .. ..	1	3	57
9—10	Milk, 10 oz.	1	1	25
10—5 A.M.	Milk, 18 oz.	8	1	50
5—6	.. .. ..	1	1	50
6—7	.. .. ..	1	0	40
7—8	Tea, 15 oz.; bread, 5½ oz.	1	0	35
8—9	Bread, 5½ oz.	1	4	40
9—10	.. .. ..	1	2	0

	oz.	dr.	min.
10 A.M. to 10 P.M.	16	2	50
10 P.M. to 10 A.M.	14	3	35
	30	6	25

Oct. 30—		oz.	dr.	min.
10—11	.. .. ..	1	3	30
11—12 noon	Chicken, 6 oz.	1	3	25
12—1	.. .. ..	1	2	0
1—2	.. .. ..	1	3	10
2—3	.. .. ..			
3—4	Tea, 17 oz.; bread	1	0	42
4—5	.. .. ..	1	4	0
5—6	.. .. ..	1	6	46
6—7	Milk, 11 oz.	1	2	0
7—8	.. .. ..	1	1	1
8—9	.. .. ..	1	2	0
9—10	.. .. ..	1	2	40
10—5	Milk, 1 pint	7	6	6
5—6	.. .. ..		7	50
6—7	.. .. ..	1	2	0
7—8	Tea, 15 oz.; bread, 5½ oz.	1	3	0
8—9	.. .. ..	1	5	1
9—10	Milk, 12 oz.	1	4	0

	oz.	dr.	min.
10 A.M. to 10 P.M.	14	7	14
10 P.M. to 10 A.M.	14	3	57
	29	3	11

Mrs. V. B. Age 42. Daily Excretion of Bile.

		oz.	dr.	min.
Oct. 31—				
10—11	.. .. ..	1	4	0
11—12 noon.	.. .. ..	1	1	20
12—1	Chicken, 6 oz.; pudding, 11 oz.; milk, 8 oz.	1	2	25
1—2	.. .. ..	1	2	35
2—3	.. .. ..	1	3	0
3—4	Tea, 10 oz.; bread, 5 oz.	1	3	0
4—5	.. .. ..	1	2	0
5—6	.. .. ..	1	1	20
6—7	Milk, 18 oz.	1	3	25
7—8	.. .. ..		7	32
8—9	.. .. ..	1	0	17
9—10	.. .. ..		7	50
10—5 A.M.	Milk, 1 pint	6	6	0
5—6	.. .. ..	1	3	0
6—7	.. .. ..	1	4	0
7—8	Tea, 15 oz.; bread, 5½ oz.	1	3	0
8—9	.. .. ..	1	3	50
9—10	.. .. ..	1	3	45

	oz.	dr.	min.
10 A.M. to 10 P.M.	14	6	44
10 P.M. to 10 A.M.	13	7	35
	28	6	19

		oz.	dr.	min.
Nov. 1—				
10—11	Milk, 12 oz.	1	0	45
11—12 noon.	.. .. ..	1	2	55
12—1	Chicken, 12 oz.; pudding, 9 oz.	1	5	0
1—2	.. .. ..	1	2	35
2—3	.. .. ..	1	4	0
3—4	Tea, 20 oz.; bread, 5½ oz.; egg, 1	1	3	0
4—5	.. .. ..	1	0	0
5—6	.. .. ..	1	3	0
6—7	Milk, 15 oz.	1	1	10
7—8	.. .. ..	1	4	0
8—9	.. .. ..	1	0	59
9—10	.. .. ..	1	1	25
10—5 A.M.	.. .. ..	7	4	30
5—6	.. .. ..	1	1	0
6—7	.. .. ..	1	0	30
7—8	Tea, 20 oz.; bread, 6½ oz.		7	0
8—9	.. .. ..	1	5	10
9—10	.. .. ..	1	4	0

	oz.	dr.	min.
10 A.M. to 10 P.M.	15	4	50
10 P.M. to 10 A.M.	13	6	10
	29	3	0

Mrs. V. B. Age 42. Daily Excretion of Bile.

		oz.	dr.	min.
Nov. 2—				
10—11	Milk, 20 oz.	1	5	10
11—12 noon	Chicken and bread, 6 oz.; pudding, 11 oz.	1	2	40
12—1	.. .. ..	1	1	0
1—2	.. .. ..	1	6	0
2—3	.. .. ..	1	2	19
3—4	.. .. ..	1	1	2
4—5	Tea, 9 oz.	1	4	0
5—6	.. .. ..	1	4	0
6—7	Milk, 15 oz.	1	2	3
7—8	.. .. ..		5	2
8—9	.. .. ..	1	0	40
9—10	.. .. ..		7	35
10—5 A.M.	.. .. ..	6	4	0
5—6	.. .. ..	1	1	10
6—7	.. .. ..		7	39
7—8	.. .. ..	1	0	35
8—9	.. .. ..	1	4	17
9—10	.. .. ..	1	4	50

	oz.	dr.	min.
10 A.M. to 10 P.M.	15	1	31
10 P.M. to 10 A.M.	12	6	31
	28	0	2

		oz.	dr.	min.
Nov. 3—				
10—11	Milk, 9 oz.	1	1	0
11—12 noon	.. .. ..	1	1	15
12—1	Chicken, 6½ oz.; pudding, 6½ oz.	1	1	15
1—2	.. .. ..	1	7	50
2—3	.. .. ..	1	0	0
3—4	Tea, 15 oz.; bread, 6 oz.; egg, 1......	1	1	30
4—5	.. .. ..	1	4	0
5—6	.. .. ..	1	4	0
6—7	Milk, 9 oz..........................		6	0
7—8	Eunonymin, gr. jss., at 7 P.M.	1	3	10
8—9	.. .. ..	1	2	0
9—10	.. .. ..	1	1	32
10—5 A.M.	.. .. ..	8	1	0
5—6	.. .. ..	1	1	40
6—7	.. .. ..	1	1	40
7—8	Tea, 11 oz.; bread. 4 oz.	1	0	0
8—9	.. .. ..	1	6	0
9—10	.. .. ..	1	4	0

	oz.	dr.	min.
10 A.M. to 10 P.M.	15	1	32
10 P.M. to 10 A.M.	14	6	20
	29	7	52

Mrs. V. B. Age 42. Daily Excretion of Bile.

Nov. 4—		oz.	dr.	min.
10—11	Milk, 9 oz.	1	2	0
11—12 noon	.. .. ..	1	1	0
12—1	Chicken, 6 oz.; pudding, 9 oz.; milk, 9 oz.	1	2	0
1—2	.. .. ..	1	5	0
2—3	.. .. ..	1	4	45
3—4	Bread, 3 oz.; tea, 16 oz.; egg, 1	0	6	0
4—5	.. .. ..	1	1	0
5—6	.. .. ..	1	4	0
6—7	.. .. ..	1	0	0
7—8	.. .. ..	1	2	0
8—9	.. .. ..	1	1	0
9—10	Euonymin, gr. iij, at 10.30 P.M.	1	0	15
10—5 A.M.	Milk, 1 pint	7	0	10
5—6	.. .. ..	1	0	36
6—7	.. .. ..	1	3	5
7—8	Tea, 10 oz.; bread, 6 oz.	1	0	45
8—9	.. .. ..	1	0	0
9—10	Milk, 9 oz.	1	0	0

	oz.	dr.	min.
10 A.M. to 10 P.M.	14	5	0
10 P.M. to 10 A.M.	12	4	36
	27	1	36

Nov. 5—		oz.	dr.	min.
10—11	.. .. ..	1	2	0
11—12 noon	.. .. ..	1	0	40
12—1	Chicken and potato, 8 oz., pudding	1	4	0
1—2	.. .. ..	1	2	25
2—3	.. .. ..	1	1	40
3—4	Tea, 16 oz.; bread, 6 oz.; egg, 1	1	1	15
4—5	.. .. ..	1	3	0
5—6	.. .. ..	1	2	50
6—7	Milk, 10 oz.	1	1	0
7—8	.. .. ..	1	0	25
8—9	.. .. ..	0	7	0
9—10	.. .. ..	1	6	55
10—5 A.M.	Milk, 1 pint	7	0	0
5—6	.. .. ..	1	0	0
6—7	.. .. ..	0	7	0
7—8	Tea, 16 oz.; bread, 4½ oz.	1	3	10
8—9	.. .. ..	1	2	0
9—10	.. .. ..	1	4	0

	oz.	dr.	min.
10 A.M. to 10 P.M.	15	1	10
10 P.M. to 10 A.M.	13	0	10
	28	1	20

Mrs. V. B. Age 42. Daily Excretion of Bile.

		oz.	dr.	min.
Nov. 6—				
10—11	Milk, 8 oz.	1	4	20
11—12 noon	Chicken and potato, 7 oz.; pudding, 8 oz.	1	2	25
12—1	.. .. ..	1	4	0
1—2	.. .. ..	1	1	50
2—3	.. .. ..	1	1	0
3—4	Tea, 10 oz.; bread, 6 oz.	1	1	10
4—5	.. .. ..	1	0	0
5—6	.. .. ..	1	3	0
6—7	Milk, 10 oz.	1	2	0
7—8	.. .. ..	1	3	0
8—9	.. .. ..	1	0	0
9—10	.. .. ..	1	0	5
10—5 A.M.	Milk, 1 pint	6	1	5
5—6	.. .. ..	0	1	30
6—7	.. .. ..	0	3	0
7—8	Tea, 11 oz.; bread, 4½ oz.	1	1	30
8—9	.. .. ..	1	5	0
9—10	Milk, 8 oz.	1	4	0

10 A.M. to 10 P.M.	14	6	50
10 P.M. to 10 A.M.	11	0	5
	25	6	55

		oz.	dr.	min.
Nov. 7—				
10—11	.. .. ..	1	2	0
11—12 noon	Chicken and potato, 8 oz.	1	0	0
12—1	.. .. ..	1	2	0
1—2	.. .. ..	1	2	0
2—3	.. .. ..	1	2	20
3—4	Tea, 16 oz.; bread, 6 oz.; egg, 1	1	2	0
4—5	.. .. ..	1	2	10
5—6	.. .. ..	1	3	10
6—7	Milk, 17 oz.	1	2	40
7—8	Calomel, gr. v	1	0	0
8—9	.. .. ..	1	0	0
9—10	.. .. ..	0	6	0
10—5 A.M.	.. .. ..	7	6	30
5—6	.. .. ..	1	0	0
6—7	.. .. ..	0	6	35
7—8	Tea, 16 oz.; bread, 4½ oz.	0	7	0
8—9	.. .. ..	1	2	0
9—10	Milk, 10 oz.	1	3	0

10 A.M. to 10 P.M.	14	0	20
10 P.M. to 10 A.M.	13	1	5
	27	1	25
At 7 P.M. calomel, gr. v, 10 hours *before*	12	6	20
10 hours *after*	10	4	30
Corresponding 10 hours *after* on previous day	9	4	10

Mrs. V. B. Age 42. Daily Excretion of Bile.

		oz.	dr.	min.
Nov. 8—				
10—11	.. .. ..	1	2	40
11—12 noon	Chicken and potato, 8 oz.; milk, 8 oz.; gravy, 1 oz.	1	0	0
12—1	.. .. ..	1	2	0
1—2	.. .. ..	1	1	30
2—3	.. .. ..	1	4	0
3—4	Tea, 19 oz.; bread, 2½ oz.; egg, 1	1	1	25
4—5	.. .. ..	1	1	55
5—6	.. .. ..	1	3	20
6—7	.. .. ..	1	2	0
7—8	.. .. ..	1	4	0
8—9	.. .. ..	1	1	0
9—10	.. .. ..	1	0	20
10—5 A.M.	Milk, 16 oz.	6	6	0

	oz.	dr.	min.
10 A.M. to 10 P.M.	15	0	10
10 P.M. to 10 A.M.	12	6	25
	27	6	35

		oz.	dr.	min.
Nov. 9—				
5—6	.. .. ..	1	1	50
6—7	.. .. ..	1	2	0
7—8	Tea, 19 oz.; bread, 4½ oz.	1	1	55
8—9	.. .. ..	1	3	40
9—10	Milk, 10 oz.	0	7	0
10—11	.. .. ..	1	1	0
11—12	Chicken and potato, 8 oz.; pudding, 8 oz.	1	1	15
12—1	.. .. ..	1	1	45
1—2	.. .. ..	1	2	0
2—3	.. .. ..	1	1	35
3—4	Tea, 12 oz.; bread, 4½ oz.	1	4	0
4—5	.. .. ..		5	0
5—6	.. .. ..	1	1	25
6—7	.. .. ..	1	1	5
7—8	.. .. ..	1	0	0
8—9	.. .. ..	1	2	0

	oz.	dr.	min.
9 A.M. to 9 P.M.	12	5	5

Mrs. V. B. Age 42. Daily Excretion of Bile.

Nov. 12—		oz.	dr.	min.
9—12 noon	Milk, 1 pint	3	4	0
12—1	Meat, &c., 16 oz.; water, 10 oz.	1	3	2
1—2	.. .. ..	1	2	2
2—3	.. .. ..	1	3	1
3—4	Tea, 20 oz.; bread, 2 oz.	1	4	2
4—5	.. .. ..	1	6	3
5—6	.. .. ..	1	6	2
6—7	.. .. ..	1	4	1
7—8	.. .. ..	1	4	2
8—9	.. .. ..	1	2	1

	oz.	dr.	min.
9 A.M. to 9 P.M.	16	6	16

Nov. 13—		oz.	dr.	min.
5—6 A.M.	.. .. ..	1	3	0
6—7	.. .. ..	1	0	10
7—8	.. .. ..		7	5
8—9	Tea, 1 pint; bread, 4 oz.	1	4	4
9—10	.. .. ..	1	4	3
10—11	Milk, 10 oz.	1	1	1
11—12	Tinct. rhei, ℥ss.		6	5
12—1	Meat, &c., 16 oz.; water, 10 oz.	1	2	1
1—2	.. .. ..	1	0	2
2—3	.. .. ..	1	0	0
3—4	.. .. ..	1	6	3
4—5	Tea, 1 pint; bread, 1 oz.	1	6	8
5—6	.. .. ..	1	0	8
6—7	.. .. ..	1	2	6
7—8	Milk, 15 oz.; bread, 2 oz.	1	0	0
8—9	.. .. ..	1	1	8
9—10	.. .. ..	1	2	0

	oz.	dr.	min.
5 A.M. to 11 A.M.	7	3	23
At 11, tinct. rhei, ℥ss.			
11 A.M. to 5 P.M.	7	4	19
9 A.M. to 9 P.M.	14	4	45
(*Cf.* 9th and 12th November and 14th and 15th November.)			
10 A.M. to 10 P.M.	14	2	42
10 P.M. to 10 A.M., not measured.			

Nov. 14—	oz.	dr.	min.
6 A.M. to 6 P.M.	16	6	37
9 A.M. to 9 P.M.	17	7	45
11 A.M. to 5 P.M.	9	6	11
12 A.M. to 6 P.M.	10	2	10

Mrs. V. B. Age 42. Daily Excretion of Bile.

Nov. 15—		oz.	dr.	min.
6—7 A.M.	.. .. ..	1	2	0
7—8	.. .. ..		6	4
8—9	Tea, 20 oz.; bread, 2 oz. ..	1	0	8
9—10	.. .. ..	1	0	5
10—11	Milk, 10 oz.	1	6	10
11—12	Tinct. rhei, ℥j	1	0	6
12—1	Meat, &c., 12 oz.; water, 10 oz.	1	2	4
1—2	.. .. ..	1	2	0
2—3	.. .. ..	1	4	2
3—4	.. .. ..	1	2	8
4—5	Tea, 20 oz.; bread, $1\frac{1}{2}$ oz.	1	0	10
5—6	.. .. ..	1	1	0
6—7	.. .. ..	1	7	3
7—8	Milk, 15 oz.; bread, 1 oz.	1	5	4
8—9	.. .. ..	1	0	5
9—10	.. .. ..	1	1	0

At $11\frac{1}{2}$ Tinct. rhei, ℥j.

	oz.	dr.	min.
6 A.M. to 12	6	6	33
12 to 6 P.M.	7	3	24
9 A.M. to 9 P.M.	15	5	57

Nov. 16—				
6—7 A.M.	.. .. ..	1	2	0
7—8	.. .. ..	1	2	8
8—9	Tea, 20 oz.; bread, 6 oz.	1	4	6
9—10	.. .. ..	1	3	2
10—11	Milk, 10 oz.			
11—12	.. .. ..	2	7	3
12—1	Meat, 16 oz.	1	2	5
1—2	.. .. ..	1	4	3
2—3	Water, 10 oz.	1	4	7
3—4	.. .. ..	1	1	5
4—5	Tea, 20 oz.; bread, 3 oz.	1	4	3
5—6	.. .. ..	1	5	4
6—7	.. .. ..	1	0	3
7—8	Milk, 15 oz.; bread, 2 oz.		7	5
8—9	.. .. ..	1	1	7

	oz.	dr.	min.
6 A.M. to 6 P.M.	16	6	46

Mrs. V. B. Age 42. Daily Excretion of Bile.

Nov. 17—		oz.	dr.	min.
7—8	.. .. ..	1	2	4
8—9	Tea, 20 oz.; bread, 7 oz.	1	6	1
9—10	.. .. ..	1	2	1
10—11	Milk, 10 oz.	1	2	3
11—12	Euonymin, gr. iv	1	3	4
12—1	Meat, &c., 16 oz.; water, 10 oz.	1	2	5
1—2	.. .. ..	1	2	0
2—3	.. .. ..	1	3	0
3—4	.. .. ..	1	2	3
4—5	Tea, 20 oz.; bread, 2 oz.	1	3	5
5—6	.. .. ..	1	4	7
6—7	.. .. ..	2	2	0
7—8	Milk, 15 oz.; bread, 2 oz.	1	1	1
8—9	.. .. ..	1	0	3
9—10	.. .. ..	1	3	0

	oz.	dr.	min.
7 A.M. to 11 A.M.	5	4	9
At 11½, euonymin, gr. iv.			
12 to 4 P.M.	5	1	8
10 A.M. to 10 P.M.	16	3	31

Hour of the day.	Jan. 18 and 19.*	Jan. 19 and 20.	Jan. 20 and 21.	Jan. 21 and 22.	Jan. 22 and 23.
	oz. dr. min.	oz. dr. min.	oz. dr. min.	oz. dr. min.	oz. dr. min.
Noon. 12—1	1 4 0	..	..	..	..
1—2	1 0 0	..	..	..	..
2—3	1 4 0	..	..	..	..
3—4	1 0 0	†1 4 0	†1 2 0	†1 2 41	1 4 10
4—5	1 0 0	1 2 0	1 2 10	1 3 0	1 3 0
5—6	1 0 25	1 2 0	1 2 40	1 2 0	†1 3 0
6—7	1 4 0	†1 3 0	†1 3 10	†1 2 40	1 3 0
7—8	1 1 0	1 1 30	1 4 0	1 2 0	1 2 0
8—9	1 2 0	1 1 30	1 1 40	†1 2 6	†1 0 40
9—10	1 0 40	†1 0 25	†1 1 40	1 2 0	1 1 20
10—11	1 4 0	1 1 0	1 4 35	1 2 40	1 2 0
11—12	1 1 0	1 1 50	7 0	†1 1 0	†1 0 45
	Jan. 19.	Jan. 20.	Jan. 21.	Jan. 22.	Jan. 23.
Midnt. 12—1	1 2 0	†1 1 30	†1 0 0	1 2 0	4 0
1—2	1 1 0	1 2 0	1 0 0	6 45	1 0 0
2—3	1 0 25	1 2 0	1 0 0	† 4 55	†1 1 30
3—4	1 1 0	†1 2 0	† 7 0	6 25	1 0 0
4—5	7 0	1 1 50	6 55	6 55	1 1 0
5—6	5 0	1 0 25	1 1 45	† 4 0	†1 2 0
6—7	5 45	†1 0 50	†1 4 0	1 1 30	1 1 30
7—8	6 0	1 0 0	1 0 0	4 20	4 0
8—9	1 2 20	1 2 6	1 6 0	† 6 0	†1 2 0
9—10	1 2 0	†1 1 0	†1 2 0	5 30	1 1 0
10—11	1 5 0	1 2 0	1 2 42	1 0 30	1 0 0
11—12	1 4 0	1 0 25	1 0 40	†1 0 20	†1 0 0
Noon. 12—1	..	†1 2 40	†1 2 0	1 4 0	1 2 0
1—2	..	1 0 0	1 6 43	1 3 40	1 2 50
2—3	..	1 1 40	1 5 30	†1 4 0	† 7 0
Total amount of bile in 24 hours	27 6 35	28 5 41	30 2 10	26 0 57	27 0 45

* No turpentine given these 24 hours.

† Means 15 min. of turpentine given at beginning of hours.

Hour of the day.	Jan. 28 and 29.	Jan. 29 and 30.	Jan. 30 and 31.	Jan. 31 and Feb. 1.	Feb. 1 and 2.
	oz. dr. min.	oz. dr. min.	oz. dr. min.	oz. dr. min.	oz. dr. min.
9—10	*1 3 30	*1 2 0	*1 1 5	*1 0 10	*1 4 15
10—11	1 3 0	1 3 0	1 3 50	1 0 20	1 2 40
11—12	1 3 50	1 0 40	1 3 30	1 0 20	1 2 0
Noon. 12—1	1 0 40	1 0 40	1 3 15	1 2 40	1 2 35
1—2	*1 2 0	*1 2 45	*1 1 45	*1 4 0	*1 4 10
2—3	1 1 5	1 2 5	1 0 10	1 1 10	1 5 5
		Not collected (say,			
3—4	1 3 0	1 0 0)	1 3 0	1 2 25	1 3 0
4—5	1 0 50	1 0 40	1 4 0	1 2 25	1 4 0
5—6	*1 0 0	*1 5 0	*1 1 0	*1 5 5	*1 4 25
6—7	1 2 30	1 0 0	1 1 30	1 0 0	1 0 0
7—8	4 0	1 0 45	1 0 40	1 0 25	1 3 0
8—9	1 3 10	1 0 20	6 0	1 0 0	1 0 40
9—10	*1 0 0	*1 2 40	*1 4 15	*1 2 0	*1 1 20
10—11	1 1 25	1 2 0	25	1 0 0	1 2 0
11—12	1 1 0	1 2 0	7 0	7 50	1 1 15
	Jan. 29.	Jan. 30.	Jan. 31.	Feb. 1.	Feb. 2.
Midnt. 12—1	1 1 20	1 1 0	1 2 0	1 0 0	1 4 0
1—2	*1 0 45	*1 3 40	*1 1 0	*1 5 20	*1 2 0
2—3	6 45	1 4 20	7 0	1 0 0	6 0
3—4	4 0	1 1 30	7 45	1 1 20	1 0 45
4—5	5 0	1 3 20	7 0	1 1 20	7 20
5—6	*1 4 0	*1 0 0	*1 0 0	*1 1 30	*1 0 35
6—7	7 50	1 1 0	1 0 0	1 2 0	1 1 0
7—8	1 1 5	1 2 10	4 45	1 2 0	1 2 0
8—9	4 20	1 4 40	1 4 10	1 2 0	1 4 40
Total amount of bile in 24 hours	26 1 5	29 6 15	26 3 5	28 4 20	30 4 45

* Benzoate of soda, gr. xv, administered.

Hourly Secretion of Bile for 48 Consecutive Hours. Normal.

	oz.	dr.	min.		oz.	dr.	min.
April 10—				*April* 11—			
11—12 noon.	1	5	30	11—12 noon.	1	5	5
12—1 P.M.	1	5	20	12—1.30 P.M.	2	4	30
1—2	1	5	25	1.30—2	1	1	5
2—3	1	4	15	2—3	1	4	0
3—4	1	5	30	3—4	1	7	10
4—5	1	3	0	4—5	2	0	0
5—6	1	3	10	5—6	2	0	0
6—7		7	0	6—7	1	6	0
7—8	1	0	0	7—8	1	4	0
8—9	1	1	0	8—9	1	5	10
9—10	1	2	0	9—10	1	3	10
10—11	1	0	35	10—11	1	2	0
11—12	1	1	10	11—12	1	5	20
Midnt. 12—1	1	4	0	Midnt. 12—1		6	45
April 11—				*April* 12—			
1—2	1	2	0	1—2 A.M.	1	2	15
2—3	1	1	30	2—3		7	0
3—4	1	2	0	3—4	1	4	55
4—5	1	0	20	4—5	1	4	45
5—6	1	1	15	5—6	2	0	35
6—7		6	0	6—7	1	5	10
7—8	1	0	0	7—8	2	0	0
8—9	1	3	30	8—9	2	1	30
9—10	1	4	0	9—10	1	6	0
10—11	1	3	25	10—11	1	6	0
	30	7	55		39	4	25

Bile Flow for 24 Hours, without Iridin. Bile Flow hourly before.

	April 13—						*April* 16—		
	oz.	dr.	min.				oz.	dr.	min.
10—11 A.M.	2	0	0	..			*1	0	0
11—12	2	0	0	..			1	0	0
Noon 12—1 P.M.	2	1	15	..			1	2	0
1—2	2	0	0	..			1	4	0
2—3	1	6	0	..			2	2	30
3—4	2	2	0	..			2	0	0
4—5	2	0	0	..			1	7	0
5—6	1	6	0	..			2	0	0
6—7	1	5	0	..			2	1	0
7—8	1	0	50	..			2	0	0
8—9	1	4	0	..			1	7	0
9—10	1	4	0	..			2	0	0
				April 15—			20	7	30
				oz.	dr.	min.			
10—11		6	35		1	15			
11—12		6	45	1	2	0			
	April 14—			*April* 16—					
12—1 A.M.		3	0		6	30			
1—2	1	3	5		2	35			
2—3	1	2	0		6	0			
3—4		5	0		6	0			
4—5	1	0	35	1	1	15			
5—6		6	15	1	6	35			
6—7	1	3	0	1	7	15			
7—8	1	1	10	1	4	0			
8—9	2	1	0	1	2	30			
9—10 A.M.	1	4	0	1	1	0	12	6	55
Total ..	34	7	30	12	6	25	33	6	25

* 10 A.M., iridin, gr. iv.

Presents, April 24, 1890.

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"If we accept the hypothesis that the elementary substances are composed of atoms, we cannot avoid concluding that electricity also, positive as well as negative, is divided into definite elementary portions, which behave like atoms of electricity." —Helmholtz (Faraday Lecture).

I. *Introduction.*

The phenomena of the electric discharge in gases excite a widespread interest at the present time. It could hardly be otherwise; for although our knowledge of electric manifestations is increasing in all directions, we cannot be assured of the correctness of our explanations while the mysterious appearance of the gas discharge remains unexplained. As long as we still have to account for a series of most puzzling facts, it seems possible that we are on the wrong road altogether, and that there may be some surprise in store for us which will ultimately compel us to reconsider all our present ideas. I have endeavoured during the last ten years to study the gas discharge, with a view to finding some explanation which should be in agreement with the conclusions drawn from other parts of physics.

In the year 1884 I presented to the Royal Society* an outline of a

* 'Roy. Soc. Proc.,' vol. 37, p. 317 (1884).

theory which seemed to me to form a hopeful starting point for future investigation. I have every reason to be satisfied with the way in which that theory has been received by other workers in the same field, and I think that, in spite of difficulties, which I do not wish to underrate, it is generally considered to have a good chance of ultimate success. I have since been able to extend my views, and am bold enough to think that we may now form a fairly complete idea of the most important features of the gas discharge.

In 1882, that is, two years before my paper was presented to the Royal Society, Mr. Giese* was led, by a study of the electric conductivity of the gases which rise from flames, to the identical hypothesis which formed the basis of my paper. I much regret that I have only recently become acquainted with Mr. Giese's work, as I should have been glad to have drawn attention to it.

We both assume that each molecule of a gas contains atoms which carry equal and opposite charges; and that these charges are the same as those carried by the ions in electrolytes; that, further, a current of electricity through a gas can only be maintained by a diffusion of the charged atoms through it. I have never claimed much originality for this hypothesis, as the same idea must have occurred to many others, and it would probably be difficult to trace its early history. The hypothesis of definite charges only forms a small part, however, of the theory which I have sketched out in my previous paper. By itself alone it is not sufficient to account for the observed facts. In order to distinguish the theory from others, I shall call it the theory of "electrolytic convection," as it resembles in many ways the kind of conduction in liquids which has been described under that name by Helmholtz. The theory of electrolytic convection offers obvious explanations of a number of different phenomena, as has been shown by Giese for the discharge through gases rising from flames, by myself for the discharge introduced by strong electromotive forces, and by Elster and Geitel for a number of other phenomena. According to this theory, a gas insulates as long as there are no free ions present, but acts as a conductor as soon as, through some cause or other, the molecules are split up into ions. We meet with formidable difficulties, however, when we come to discuss the transference of electricity between the solid conductor and the gas; for here we are to a great extent tied down by our knowledge of electrolysis in liquids. We know that a difference of potential of 2 volts between the electrodes is sufficient to decompose water, and therefore to allow the interchange of electricity between the metal and the ion of hydrogen or oxygen. No theory of gas discharges is admissible which cannot be brought into harmony with these facts.

* 'Wiedemann, Annalen,' vol. 17, p. 537 (1882).

Definition of Unipolar Conductivity.

An electrode often discharges electricity of one kind more freely than that of the opposite kind. The phenomenon has been called that of unipolar conductivity, but we evidently require some understanding as to when we shall call a gas or an electrode positively unipolar. If a positive charge on an electrode becomes more quickly dissipated than a negative charge, we may either say that the electrode discharges positive electricity more rapidly into the gas, or that the gas discharges negative electricity more rapidly into the electrode. Some confusion may be caused by the fact that different writers have looked at the question from different sides and have not adopted a uniform nomenclature. I shall call an electrode positively unipolar if it discharges positive electricity more freely into the gas than negative electricity.* A great part of the following paper will deal with the circumstances under which the exchange of an ion between the conductor and the molecules or atoms takes place, and it will be shown how the peculiar behaviour of gases may be explained. This I consider an important step in support of the theory.

Such expressions as "surface resistance" or "counter electromotive force" have often been employed in cases where they seem to me to be quite unsuitable, and I shall therefore avoid them altogether; but I may occasionally briefly speak of an impediment to the passage of electricity at an electrode, meaning that a rapid fall of potential at that electrode is required to introduce or maintain the discharge.

Circumstances under which the Discharge takes place.

It will be convenient to divide the subject into two main parts: the one dealing with the circumstances under which a discharge can take place, and the other with the phenomena of the discharge itself. It is well known that, if one of two conductors, separated by a gas, is electrified while the other is connected to earth, the gas acts as a perfect insulator as long as the difference of potential between them does not exceed a certain value; if it does, the dielectric strength of the medium breaks down.

It is a matter of some doubt whether, if a spark passes between two conductors of different form, as for instance between a sphere and a plane or between a point and a sphere, the striking distance is different in the two directions. There probably is some difference of this nature in favour of a more easy escape of positive electricity,

* Elster and Geitel in a recent publication have adopted a different nomenclature. They say that a glowing platinum wire in oxygen discharges negative electricity more easily than positive, meaning that the electrode allows a free passage to negative electricity from the gas to the electrode ('Wiedemann, Annalen,' vol. 38 (1889), p. 39).

though the want of symmetry is not very marked. The important measurements of Wiedemann and Rühlmann* are generally taken to prove a more easy escape of negative electricity under reduced pressure; but when a number of sparks are taken in rapid succession, as they were in these experiments, recent researches seem to indicate that the gas and electrodes may not return to their original condition between the discharges. The consequence is that such a rapid succession of sparks shows some of the phenomena of the continuous current, and the want of symmetry is therefore of the same nature as that which belongs to the discharge, and has nothing to do with the circumstances which determine whether a discharge shall pass or not. The same remarks apply to Lichtenberg figures and Priestley's rings; they are complicated cases of phenomena which are best studied by means of the continuous current. I pass on to consider the cases where a current may pass between two electrodes, the difference of potential being small.

We have four different ways of converting the gas into a conductor, and our subject must be subdivided accordingly. If the electrodes are either (1) heated to redness or (2) illuminated by ultra-violet light, the discharge passes even when the difference of potential does not amount to more than a few volts. Further (3), it has long been known that flames do not behave as dielectrics; and, finally (4), gases through which a discharge is passing have been shown to conduct freely.

(1.) *Discharge from Glowing Electrodes.*

Edward Becquerel,† as far as I know, was the first to discover that air between red-hot platinum electrodes ceases to insulate, and he points out that when the two electrodes are of different sizes, the equalisation of potential takes place more quickly if the larger electrode is negative. Guthrie,‡ by a different method of experimentation, found that red- or white-hot iron balls discharged electricity more easily when positive than when negative.

Looking at the question from the point of view of the theory of electrolytic convection, it seems of the greatest importance to decide whether the discharge from hot bodies is accompanied by any chemical action or not. Mr. Arthur Stanton has conducted in my laboratory a research intended to throw light on the question. The experiments were planned and independently carried out by him, and his own account of them appears (p. 559) as an Appendix to this paper. But I must briefly allude to his results, which I consider to be of great importance.

* 'Poggendorff's Annalen,' vol. 145, pp. 235, 364 (1872).

† 'Annales de Chimie,' vol. 39, p. 355 (1853).

‡ 'Phil. Mag.,' vol. 46, p. 257 (1873).

Behaviour of Copper Electrodes.—Let us first consider the case of conductors charged negatively. Clean copper, heated to a full redness in air, is found to discharge negative electricity freely, but, if oxidised carefully all over the surface, it will retain its charge. If on the other hand oxidised copper is heated in hydrogen instead of in air, it will be quickly discharged until all the oxide is reduced; after that, the charge will be retained. In other words, negative electricity is quickly discharged during the process of oxidation or deoxidation, but not otherwise, as when clean copper is heated in hydrogen or oxidised copper in air. So far as the experiments could be carried out with iron in place of copper they gave similar results. As regards the behaviour of copper when it is positively electrified, the phenomena are not so sharply defined. Positive copper in an atmosphere of hydrogen seems certainly to discharge itself, even when perfectly reduced, so that it retains a negative charge. Copper in air seems to discharge positive electricity less freely when it is oxidised than during the process of oxidation, but whether it ultimately retains its charge in all cases or not has not yet been completely determined. Preliminary experiments in nitrogen have given the same results as those observed in hydrogen, leakage taking place when red-hot copper is electrified positively, but not when it is electrified negatively.

These experiments tend to show that the process of oxidation or deoxidation can powerfully affect the facility of discharge from red-hot iron or copper; so much so that no very extraordinary precautions are necessary to destroy all signs of leakage of a negative charge. We are not justified as yet in asserting that the discharge of negative electricity is *always* accompanied by a chemical action even with the metals experimented upon, for the temperature at which the experiments were carried on was limited by the danger of melting the copper, and the leakage was only tested by the more or less rapid collapse of two gold leaves connected with the charged body. A higher temperature might have brought out the leakage again, or even at a red heat more delicate methods might have shown that the leakage was only greatly reduced, but not destroyed. As regards positively charged bodies, it is still an open question whether chemical action plays any part in regulating the rate of discharge. The gold leaves collapsed quickly when copper was heated in hydrogen, but there may have been some action between the two bodies, and Mr. Stanton noticed the fact that the copper became very brittle. It is possible that with pure gases not acting chemically on the electrodes no discharge at all takes place, but this is by no means proved, and on the whole I think the evidence is against the supposition. We must for the present be satisfied with the fact that we can actually trace the effect of chemical action in destroying the leakage of negative electricity.

Behaviour of Platinum Electrodes.—We owe to Messrs. Elster and Geitel* a series of most instructive experiments, in which they have discussed the unilateral conductivity of gases in several cases. Their research was chiefly directed to an investigation of some observed electromotive forces between glowing bodies and the surrounding gas. These electromotive forces, as well as those discovered by Righi, which appear under the action of ultra-violet light, will, I believe, prove to be of the greatest importance in clearing up many electrical questions, for they seem to me to belong to a class of phenomena which can be brought under the domain of the second law of thermodynamics. I am, however, for several reasons, anxious to avoid, for the present, a discussion regarding them, except in so far as they seem to have a bearing on the question of a possible chemical action at the electrodes. The facts discovered by Elster and Geitel are briefly as follows :—

If a white-hot platinum wire and a cold metal are brought near each other in an enclosure filled with gas at various pressures, a difference of potential is observed between the two metals; when the gas is air or carbonic acid, the hot platinum is electro-negative to the cold wire, while in hydrogen or hydrocarbons the opposite is the case. It is also found that platinum in air discharges positive electricity more readily than negative, while again the opposite is the case in hydrogen.

Messrs. Elster and Geitel bring these facts into connexion with each other. The curious relationship between unilateral conductivity and electromotive force, which they have traced, is an important one, but may only be one of those reciprocal relations (Peltier effect and thermo-electric force) so often met with in all parts of physics. As regards the explanation of these phenomena, several considerations occur to me which should make us careful not to pronounce a definite opinion at present. The authors take it for granted that the electromotive force has its seat at the hot junction of metal and air. That is probable, but it will be safer for the present not to commit ourselves to more than is actually observed, namely, that if a cold platinum wire in air is placed near a hot platinum wire, the hot wire will be electro-negative towards the cold metal, while in hydrogen it will be electro-positive. At first sight the behaviour of platinum wire seems exactly the reverse to that of copper; but platinum shows some curious effects on long continued heating, which lead me to the opinion that complicated effects here determine the nature of the phenomenon. Elster and Geitel have examined the changes in the behaviour of a platinum wire kept glowing in hydrogen. Two platinum wires were kept in the same enclosure; one of them was

* 'Wiedemann, Annalen,' vol. 19, p. 588 (1883); vol. 31, p. 109 (1887); Vienna Academy 'Sitzungsberichte,' vol. 97 (Abth. 2. a.), p. 1175 (1888).

heated only during as short a time as possible, while the other was kept glowing for several hours. The former, while hot, became electro-negative towards the gas in contact, while the latter became electro-positive. It seems probable that the difference is due to gases dissolved in the platinum and given up by long-continued heating, and we are led to the opinion that in the normal state of platinum, when it is freed from its dissolved gases, it becomes electro-negative on heating. The authors do not state whether the unilateral property of hot platinum altered together with the sign of its electrification, though their own experiments would lead one to think that this was the case.

It is unnecessary to enter into the question how the dissolved gases can affect the result. We observe very similar phenomena with platinum electrodes in liquids. The great differences observed, owing to comparatively small changes in the state of the platinum, incline me to a chemical, rather than to a physical, explanation. An important experiment bearing on the question is worth quoting. Berliner* showed that the disintegration of platinum electrodes, so commonly observed in vacuum tubes, altogether ceases when the metal has been deprived of its dissolved gases. While disintegration is taking place the surface is constantly renewed, and any surface action between the platinum and the surrounding gases could be kept up continuously, but would stop as soon as the disintegration itself has stopped.

Similar effects to those described by Elster and Geitel had been previously observed by Goldstein and Warburg. After Hittorf† had discovered that the fall of potential at the negative electrode, which is necessary to produce a continuous discharge, disappears if a glowing platinum wire is taken as kathode, Goldstein found that the platinum wire permanently loses the power of acting if it is kept red-hot for a sufficient time. Warburg‡ states that, on hardening the wire by drawing it may be brought back to its original condition, so that on heating it will again discharge negative electricity freely. But I learn from private information which Professor Warburg has been kind enough to supply me that the surface of his wire might have become coated with greasy matter in the act of hardening, and, if that is so, we are at liberty to connect the free discharge of negative electricity with a chemical action at the electrode.

Behaviour of Carbon Electrodes.—It is clear that the behaviour of carbon electrodes cannot give us any certain information on the question as to how far unipolar effects depend on chemical action; but, as some phenomena shown by glow lamps have received the attention of electricians, it may be useful to point out that, as far as

* 'Wiedemann, Annalen,' vol. 33, p. 289 (1888).

† 'Wiedemann, Annalen,' vol. 21, 1884.

‡ 'Wiedemann, Annalen,' vol. 31, p. 592 (1887).

I can see, they present no special features which are not easily explained by well-known facts of vacuum discharges. Owing to the incandescence of the filaments, the different potential at the ends is sufficient to produce a discharge through the residual gas; hence the observed disintegration of the negative end of the filament. If a separate metallic plate is inserted into the lamp, its potential will be intermediate between that of the positive and that of the negative pole. On being connected with the positive pole a current will tend to pass in such a direction as will make the plate an anode. The current actually will pass, because we know that a conductor offers no difficulty to the discharge of positive electricity into a gas in which the discharge is established. This phenomenon has been called the "Edison effect." If the plate is connected with the negative electrode, it will tend to become a kathode. The fall of potential required is now much greater, and hence the current is much weaker, and may escape detection.*

According to the theory of electrolytic convection, we conclude that when an electrode is raised to a red or white heat the molecules partially dissociate and render conduction possible. It does not necessarily follow that the temperature alone is sufficient to dissociate the molecules; it may be that the addition of an electric stress is necessary. It will be a matter for future investigation to decide whether the decomposition goes on at the same rate in contact with a negative wire as it does in contact with a positive wire.

2. *Behaviour of Electrodes illuminated by Ultra-violet Light.*—Hertz,† during his celebrated experiments on electric oscillations, noticed that a spark passed more easily between metallic points when these were illuminated by a simultaneous strong spark from another source, and traced the effect to the illumination of the secondary electrodes by the ultra-violet rays sent out by the primary spark. Messrs. E. Wiedemann and Ebert‡ found that the negative electrode only is active in this case, and Hallwachs§ subsequently attacked the question by a method which has yielded very interesting results.

He connected a gold leaf electroscope with a clean plate of zinc, upon which a beam of strong ultra-violet light was allowed to fall. The zinc was found to be incapable of retaining a negative charge; the effect on a positive charge was so small that at first sight it seemed totally absent. But the fact to which I wish to draw special attention is the importance, in order to ensure the success of the experiment, of taking a clean surface of zinc. Hallwachs found that

* [All the facts described by Mr. Fleming in his recent communication ('*supra*, p. 118) will be found to agree with this explanation.—May 30.]

† 'Wiedemann, Annalen,' vol. 31, p. 983 (1887).

‡ 'Wiedemann, Annalen,' vol. 33, p. 241 (1888).

§ 'Wiedemann, Annalen,' vol. 33, p. 301 (1888).

zinc exposed to the air for some time showed the negative leakage to a much smaller extent, but the positive leakage was not affected, so that old surfaces sometimes discharged a positive charge more easily than a negative one. Other metals behave like zinc, but the less oxidisable ones show the effect to a much smaller extent.

Hallwachs draws the conclusion, to which indeed his experiments seem inevitably to lead, that the ultra-violet light produces a chemical change at the surface, which is of the same nature as the one going on when surfaces are left lying exposed to the air. The explanation which at first sight seems the most natural one, namely, that the chemical change is an oxidation, would appear from an experiment more recently communicated by him* not to be the true one, for copper covered with a film of oxide behaves like clean metallic copper.

Shortly after the publication of these experiments, Righi† showed that a photo-electric air-cell can be constructed by taking two parallel plates of different metals with an air space between them and connecting them with each other. If one of the plates is of zinc, the other of brass, and if the zinc plate is illuminated, a current tends to flow from the zinc to the brass through the air.

In the actual experiments, the brass plate was in the form of a wire netting, so that the rays from an arc lamp could pass through the grating and fall normally on the zinc plate, and the difference of potential was observed by means of an electrometer. The energy of ultra-violet light vibrations must therefore directly or indirectly be converted into that of electric separation, and it is to my mind natural to assume that a chemical action is set up in the first place by the light, and that the observed electrical effects are therefore in the first instance due to chemical action. The fact that the electromotive force of such a photo-electric cell is in the same direction as if the plates were immersed in water, the zinc becoming the anode, is very suggestive. At present, however, we must not attach too great an importance to it. I consider it quite possible that if the oxygen of the air is replaced by hydrogen, the currents would still be in the same direction. Stoletow‡ has examined the intensity of currents sent from an outside electromotive force through an interval between two parallel plates acting as electrodes, the kathode being illuminated by ultra-violet light. I cannot at present attach any very great importance to the experiments made with different gases. We know how very delicate the surface actions on which these currents depend are, and how the greater part of the effect may in some cases be due

* 'Wiedemann, Annalen,' vol. 37, p. 666 (1889).

† 'Phil. Mag.,' vol. 25, p. 314 (1888).

‡ 'Comptes Rendus,' vol. 107, p. 91 (1888).

to small traces of foreign gases or to the state of surface of the electrode.

The modifications of the disruptive discharge between platinum electrodes, which have been studied by Wiedemann and Ebert,* deserve to be mentioned, but they depend on too many circumstances to help us in deciding the question as to the possible effects of chemical action at the surface of the electrodes. In order to obtain an insight into these questions, it seems better to study the discharges produced by radiation in cases where naturally a discharge would not occur. This will probably lead to a simpler result than an investigation of the modification by radiation of a discharge independently produced. The interesting facts discovered by Lenard and Wolf will be mentioned in connexion with another part of our subject.

Discharges through Flames.

It was discovered by Paul Erman† towards the beginning of this century that a flame may conduct, and that a wire placed inside it discharges positive electricity more easily than negative electricity. Buff‡ found that even the gases rising from a flame behave as conductors of electricity, and Giese has carefully examined the behaviour of these gases. He was thereby led, as has already been mentioned, to the theory of electrolytic convection. It seems indeed impossible to draw a different conclusion. We may assume that in a flame a certain proportion of dissociated ions are present, some of which can escape combination with each other for some time. As the gases rise from the flame they will remain conductors until the recombination of ions is complete, and this may take some time. According to Giese, the gases may preserve their conductivity for some minutes.

Dissociation alone, it must be remembered, is not sufficient to convert a gas into a conductor. Thus, when owing to increased temperature the molecules of iodine vapour split up, the atoms do not necessarily behave as ions, for they are probably unelectrified. We must imagine each iodine atom in the molecule to possess two charges, and as dissociation proceeds a re-arrangement of the charges takes place, so that the free atom will still carry two equal but now opposite charges.

Discharge through Gases in the Sensitive State.

It is convenient to say that a gas is in its sensitive state as regards electric stress when any electromotive force, however small, pro-

* 'Wiedemann, Annalen,' vol. 33, p. 241 (1888).
† 'Gilbert, Annalen,' vol. 11, p. 150 (1802).
‡ 'Annalen der Chemie,' vol. 80, p. 1 (1851).

duces a transference of electricity. I have shown that a discharge, whether disruptive or continuous, throws the whole enclosure into a sensitive state. It has been suggested from various sides that some of the phenomena described by me were due to illumination of ultra-violet light. In order to decide the question, I have divided a vacuum tube into two parts by means of a quartz diaphragm; a discharge passed through one part of the tube and on that side the effects previously described by me were again observed, but the quartz plate completely destroyed the effect in the other compartment, while all observers agree that quartz is transparent to the radiations which produce electrical effects.

Because E. Wiedemann* finds (in accordance with Hallwachs' experiment) that the leaves of an electroscope can be made to collapse when they are illuminated by an arc lamp, it hardly follows, as implied by him, that this is the explanation of the same effect observed by me when there was no arc lamp nor any light except the feeble glow of a discharge carefully screened off from the gold leaves by a metallic plate. The above-mentioned experiment, which will be described in detail in another communication, disposes of the question, and shows that, independently of any ultra-violet radiation, the discharge puts the gas into a sensitive state in which it becomes a conductor. According to the theory of electrolytic convection, this should be so, because the primary discharge supplies the necessary ions.

Discussion of Unipolar Effects.

It is of interest to see whether we can trace any regularity in the appearance of the unipolar effects, and whether there is any hope of a general law which will allow us to predict when an electrode will be positively, and when it will be negatively, unipolar.

I am aware of the dangers of premature generalisation, but, at the same time, such generalisations as a rule do no harm except perhaps to the reputation of the author; they have, on the other hand, the advantage of putting the issue clearly before the reader, and often lead more quickly to a definite result than a mere statement of disconnected facts.

As far as the facts go at present, I am inclined to draw the following conclusion:—*A free discharge of electricity between a negative ion and the anode is possible. On the other hand, a considerable fall of potential is required in order to produce an exchange of electricity between a positive ion and the kathode unless the electrode takes part in a chemical action set up at its surface, in which case the rate of exchange of charges at the kathode may become greater than that at the anode.*

* 'Wiedemann, Annalen,' vol. 35, p. 219 (1888).

A word is necessary to explain what I mean by a considerable fall of potential at the electrode. In electrolytes the fall of potential within the double layer is of the order of magnitude of 1 volt; such a fall in itself would hardly make itself apparent in our measurements in gas discharges. The observed fall of potential in vacuum tubes at the kathode is measured in hundreds of volts, but it will appear that within molecular distances the fall is many times smaller than for electrolytes, so that the normal force at the kathode may be less in gases than in electrolytes. The difference between an electrolyte and a gas is this: that, while in a liquid the fall of potential only takes place within a molecular range, in the gas it continues through a measurable distance. The cause of this, no doubt, has to be explained, and an attempt will be made to do so. At present it is sufficient to point out that a large fall of potential within, say, a centimeter of the electrode is not inconsistent with the much smaller fall observed within the double layer of liquids. It must also be kept in view that all measurements of potential in the gas are made by means of secondary electrodes. If a double layer of the same moment covers both the kathode and the secondary electrode, it would escape detection. There are some delicate questions involved in this which will be discussed in the complete paper. The main conclusions drawn in the text are not altered by the fuller discussion of the problem. A few facts may be quoted in support of the view I have taken concerning the effect of chemical action at the kathode. A flame conducts, because in the flame the molecules are broken up by independent chemical action; we find, in consequence, that an electrode can freely discharge positive electricity. On the other hand, a glowing piece of charcoal may act as electrode, because it takes part in a chemical action, but it is the negative electricity which now escapes freely.

In a vacuum tube, whatever chemical action may take place at the kathode diminishes the fall of potential, as appears from Warburg's* observations, who finds that electrodes of zinc and copper in hydrogen, when first introduced and therefore probably covered with a film of oxide, show a considerably smaller fall of potential than after they have been used some time, when the oxide film may be supposed to have been removed. In a similar manner a small admixture of moisture in nitrogen tubes, causes a considerable reduction in the fall of potential. As regards red-hot copper wires, we have seen that the passage of negative electricity from the electrode to the gas for small electromotive forces may be prevented altogether, if chemical action is prevented, while the electrode may still act as anode. Here it is the combined effect of temperature and electric stress that splits up the molecules into ions.

* 'Wiedemann, Annalen,' vol. 31, p. 592 (1887).

Finally, the action of ultra-violet light, being probably of the nature of a chemical action, at once destroys the impediments which prevent metals acting as kathodes. The principal difficulty of the explanation lies in the behaviour of white-hot platinum in hydrogen, but, as has already been explained, the platinum seems to undergo a molecular change in that case, which may either be due to or equivalent to a chemical action.

Properties of Gases in their Sensitive State.

A gas may be put into a sensitive state when the molecules are broken up into ions either by a discharge of electricity or by a chemical action as in a flame. It is interesting to note the similarity of behaviour of gases made sensitive in these two ways.

In a footnote to the paper, in which the theory of electrolytic convection is brought forward by Giese, he suggests that the power of gases rising from flames to condense moisture, as shown by Aitken, may be due to the same cause as its power to conduct electricity. The question has been treated with great ability by R. v. Helmholtz, whose untimely death at an early age science has recently had to deplore. R. v. Helmholtz was led to the enquiry by the discovery of an effort of electric discharges to act in the same way as solid nuclei would in precipitating the aqueous vapour from a moist gas. It would seem natural at first sight to ascribe this effect, as well as those previously described by Aitken, to the actual presence of dust particles. I must refer the reader to Helmholtz's paper,* in which this hypothesis is finally found to be insufficient and in which the action of both flames and electric discharges is ascribed to the presence of ions in the gas. Messrs. Lenard and Wolf† have recently shown that when a metal, charged negatively, is exposed to ultra-violet light, the space in front of the discharging plate acts on a steam jet like dust particles would. They conclude from these researches that the ultra-violet light does actually disintegrate the surface of the conductor. Some of their experiments, however, admit of a different interpretation; the electric discharge itself, which we know to take place under the circumstances, might be the cause of the condensing power of the air. As far as the evidence goes, I should say that the explanation given by Lenard and Wolf is the more probable one, though it is not altogether proved as yet. But even admitting the presence of dust particles thrown off from the conductor, it is still an open question whether the disintegration of the electrode is the *cause* of the electric discharge or whether it is only one of the phenomena attending it. We should expect a con-

* 'Wiedemann, Annalen,' vol. 32, p. 1 (1887).

† 'Wiedemann, Annalen,' vol. 37, p. 443 (1889).

siderable amount of energy to be required to convert the metal surface partially into dust, and it seems doubtful whether the mere absorption of ultra-violet light is sufficient for the purpose. If the action of the light is primarily a chemical one, the disintegration might be explained, like the discharge itself, as a consequence of this chemical action.

The fact that flames behave as diamagnetic bodies has long been known, and could be explained if we were free to assume that ions are diamagnetic. From the point of view of Weber's theory of diamagnetics, this seems probable, and I made a few experiments in order to prove in other ways the diamagnetism of ions. I have not hitherto obtained decisive results, but some effects of magnets on the discharge described in my previous Bakerian Lecture seem to me to point directly to such a diamagnetism.*

From the point of view, then, of our theory, a gas is sensitive electrically when it contains free ions. On the other hand, we must conclude that when a gas is not sensitive such free ions are not present. I do not see how the insulating power of air at the ordinary temperature is consistent with the presence of ions, however few in numbers; for, ultimately, a diffusion to the electrodes and a discharge would necessarily take place. This seems to me to be fatal to J. J. Thomson's view of the disruptive discharge.†

The Continuous Discharge.

Facts hitherto established.

The first step towards the investigation of the continuous discharge is the investigation of the relation between current and fall of potential in a gas through which a current is passing. An important fact discovered by Hittorf‡ relates to the fall of potential in the positive part of the discharge, that part which extends from the anode towards the negative glow. Hittorf's law may be stated as follows:—

"*In the positive part of the discharge the rate of change of potential at a given pressure is independent of the current.*"

The law was tested by Hittorf with currents which differed in intensity nearly in the ratio of 1 to 50.

As regards the negative glow, Hittorf chiefly experimented with electrodes in the form of wires running parallel to the axis of his tubes. The glow for weak currents only covers the ends of the electrode, but as the intensity is increased it gradually extends backwards; during this stage the rise of potential near the kathode

* 'Roy. Soc. Proc.,' vol. 37, p. 317 (1884).

† 'Phil. Mag.,' vol. 15, p. 427 (1883).

‡ 'Wiedemann, Annalen,' vol. 20, p. 705 (1883).

is independent of the current. When the glow has completely covered the kathode, a rise in the intensity of the current is accompanied by an increased difference of potential between the kathode and the surrounding portions of gas.

Warburg* has studied the fall of potential near the kathode with electrodes made of different material and at different pressures. It is known that the negative glow consists of three parts: a luminous layer directly adjacent to the kathode, the so-called dark space, and the glow proper. As the density of the gas diminishes these layers gradually expand; but the difference of potential only changes slowly. It has been stated that the difference of potential between a kathode which is not completely covered with the glow and some point in the glow is independent of the intensity of the current; Warburg calls it the normal fall of potential. While the fall is normal a change of current is only accompanied by a change in the extent to which the kathode is covered with the glow. Warburg has shown how the normal fall depends on the material of the electrode, the nature of the gas, and the density, and his results are of considerable interest from our point of view. Slight quantities of moisture added to nitrogen produce a considerable change in the difference of potential. Thus, with platinum electrodes nitrogen containing a small quantity of aqueous vapour gave a normal fall of 260 volts, while dry nitrogen gave 410 volts. This is not due to the fact that aqueous vapour is a better conductor than nitrogen, for, if more than traces of moisture are present, the fall of potential is as great or greater than with dry nitrogen. We must, on the other hand, find in these observations a strong support of our view, that the difficulty of passage of positive electricity from the gas to the electrode is much reduced by chemical actions at the electrodes. The effects of small traces of moisture are so similar to those investigated by Dixon and others in chemical reaction between gases and between a solid and a gas, that in my opinion we really have no choice but to adopt some such view as I have taken of the matter. Warburg draws the same conclusion from his experiments.

The effects of deoxidation of the electrodes, where zinc or steel kathodes are used in hydrogen, makes itself apparent by considerable reduction of the fall of potential near the kathode, which disappears when the layer of oxide on the kathode has been removed by disintegration of its surface. A change of pressure, according to Warburg, is only accompanied by a slight change in the normal fall of potential.

* 'Wiedemann, Annalen,' vol. 31, p. 592 (1887).

Volume Electrification near the Kathode.

I have spent considerable time during the last few years in investigating the fall of potential in the different parts of the negative glow with a view to deciding whether there is any considerable volume electrification of the gas itself.

De la Rue and Müller* have already endeavoured to measure the potential at different places in the dark space; but for some reason, which I cannot trace, their results are altogether anomalous. They used what they call four different arrangements of leads, which, however, all should give identical results; but no regularity can be traced. In many cases they found that the potential becomes more and more *negative* away from the kathode, so that in one of their observations some point in the dark space was at a potential -1068 volt, the negative electrode being at potential zero. I have never observed anything but a regular increase of potential within the dark space. In order to be able to subject the results of experiments to a simple analysis, I have used two different kinds of vessel. In one the lines of flow were, throughout, parallel to each other; this I shall call the axial discharge. Cylindrical tubes were used, and the kathode was a closely fitting aluminium plate, care being taken to prevent a discharge from the back. The other vessels used were large cylindrical receivers, the kathode being a wire forming the axis of the cylinder, and the anode a cylinder made of wire gauze. The lines of flow in these vessels were radii drawn from the axis of the cylinder, and I shall call the discharge in this case the radial discharge. The fall of potential between the kathode and any point inside the negative glow was found to be capable of being represented by the formula

$$V = V_0(1-e^{-\kappa x}) \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (1.),$$

where V is the potential at any point, V_0 the potential in the glow proper, κ a constant, and x the distance from the kathode. The potential of the kathode is taken as zero. The formula is not meant to be more than an approximate one, but within the limits of error of experiment the formula may be taken as correct through the dark space and the inner parts of the glow. It may, in the first place, only be considered as an interpolation formula, and others might be found representing the facts equally well, but the equation gains some reality owing to the fact that κ for a given pressure is found to be very nearly independent of the strength of the current. The complete account of my experiments will show how far this is correct. Owing to the circumstances of the case, the fall of potential

* 'Phil. Trans.,' 1883 (vol. 174, p. 477).

could not be altered more than in the ratio of one to two, and it is not possible to tell, therefore, how far the above will express the results for a greater change in the current density. The constancy of κ depends on the fact which I have verified within these limits of currents, that *the potentials at different points of the negative glow all rise and fall in the same ratio when the current is altered, the kathode being at zero potential.*

From the characteristic equation for the potential, which in our case reduces to

$$\frac{d^2V}{dx^2} = -4\pi v^2 \rho,$$

in which v stands for the velocity of light, we may now deduce ρ, or the volume-density of electricity near the kathode.

The law which I have given above suggests at once that ρ is a linear function of V or its differential coefficients, for it implies that if V is any solution of the characteristic equation, λV must also be a solution. The question will be discussed in the complete account of these experiments, and it is not necessary to enter into it here. From equation (1) we derive

$$4\pi v^2 \rho = \kappa^2 V_0 e^{-x\kappa} \quad \ldots\ldots\ldots\ldots\ldots\ldots (2.),$$

which shows that *the kathode is covered with an atmosphere of positively charged particles diminishing outwards in volume-density.*

The law of variation of density is the same as that found in the atmosphere near the earth's surface; but the mathematical conditions are very different. The gravitational force near the earth is sensibly constant, while the electrical forces near the kathode vary as much as the density.

If the curve which connects V and x is plotted, its curvature, and therefore the electrification, can be traced through the dark space and into the negative glow, but inside the glow it rapidly diminishes. The formula for ρ does not lay claim to more than an approximate expression of the facts, which may, however, in default of more accurate knowledge help us to form some idea of the distribution of volume-density. Even though (1) may hold with considerable accuracy, (2) may not give correct results for those parts of the glow which are close up to the electrode; for the curve representing V near the origin is a very steep line, slightly curved. A small change in the curvature will make a considerable change in ρ, without affecting the main curve to an appreciable extent. It seems probable, however, that the electrification continues to increase up to the electrode itself, and that the formula will express the main features of the distribution. When the lines of flow are radial the law of distribution of volume-density is less simple, but the general result is the same.

It is interesting to gain some idea of the charges in absolute units which are involved, and of the number of charged ions taking part in the discharge according to our theory. The electrification at the surface according to (2) is $V_0\kappa^2/4\pi v^2$. In a series of experiments in which V_0 varied from 672 to 1330 volts, and the current between 400 and 3000 microampères, $\kappa^2 V_0$ was found to vary between 3×10^{12} and 10^{13} C.G.S. units. Taking the highest value, we find $\rho = 10^{-9}$ C.G.S. electromagnetic units, or about 30 electrostatic units. From this I calculate that the *mass* of electrified atoms per unit volume is 7×10^{-13}, on the supposition that the charges are carried by nitrogen-atoms. The density of nitrogen in the experiment which forms the basis of this calculation is 5×10^{-7}, so that even close up to the electrode the surplus of positive ions is very small compared to the number of atoms and molecules present, only about one molecule in a million being decomposed. Further away from the kathode the relative amount of charged to uncharged particles is considerably smaller still. The volume-density, which is derived from calculation, gives us, of course, the difference only between the sum of the positive and the sum of the negative charges. The value of this excess of positive charges cannot therefore be taken as a measure of the total number of ions present, except perhaps close up to the electrode. In how far the positive charges in the polarising layer and the negative charges projected away from the kathode are *alone* sufficient to account for the whole current, cannot be decided at present. The mutual repulsion of the particles within the atmosphere of positive particles which surrounds the kathode explains at once the tendency of the glow to spread all over the surface of the electrode. Other important facts, such as the effects of an anode near the kathode in driving away the glow, will also find their natural explanation. The spreading of the glow over the negative electrode is the cause of a series of peculiar differences which appear at the two poles even in the case of discontinuous discharges. The differences observed in Lichtenberg's figure and in Priestley's rings, according as the discharging point is positive or negative, seem to me to be readily explained by the atmosphere of positive ions which always tends to spread all over the kathode, while the discharge from a positive electrode is confined to the points of maximum electric density. It will lead me too far to enter into these questions here; but one objection must be met: why does this positive electrification not make itself apparent by electrical action in the space outside; and why do electrified bodies outside not act on the glow?

As regards the first question, the charge is not really a large one. The whole quantity of electricity in the glow per square centimeter cross-section would, according to the formula, be—

$$\frac{dV}{dx}\Big/4\pi v^2,$$

where dV/dx stands for the fall of potential at the kathode. In my experiments this quantity has been smaller than 10^{12} C.G.S., so that the maximum total charge per centimeter cross section would amount to about three electrostatic units. The effect of this electrification would be completely hidden by the charges on the outside of the glass tube, which cannot be avoided when we are dealing with a thousand volts. In fact, the leakage over the glass will act as an electrical screen.

It also appears that electrified bodies outside cannot produce any effect on the inside of the tube, because it can be shown from my previous work that the surface conditions in the interior of a gas through which a discharge passes render a normal force at the surface impossible. Hence no lines of force can pass from the outside to the inside. If an electrified body is approached, a momentary redistribution of the surface charge inside the vessel will take place; but after the readjustment no further effect can be observed.

It is generally stated that a greater electromotive force is required to start a discharge than to maintain it after it is once started. This statement requires qualification. The fall of potential in the positive part of the tube is of course much smaller during discharge than it was just before, but it does not follow that the rate of fall at the negative electrode is less. In one experiment the rate of fall at the kathode at a pressure of 6 mm. was 9000 volts per centimeter, while the whole of my battery only had a difference of potential of less than 2000 volts. According to De la Rue and Müller, it requires a fall of about 1800 volts to start a discharge between two parallel plates at a pressure of 10 mm. Their measurements do not extend to smaller pressures, but a discharge would be started at the pressure of about $\frac{1}{2}$ mm. by a less electromotive force than at a pressure of 10 mm. It follows that while the discharge is passing, in most if not in all my experiments, the fall of potential was much greater than that required to start it, and if we adopt the view that the breaking down of the insulation is in part due to a decomposition of the molecules, it follows that the molecules must continue to be decomposed during the discharge. This we shall find to be a result of importance.

The charge on platinum electrodes in liquids is much greater than the charge on the surface of the kathode in a gas; with the usual data I make it about 10,000 times greater. This would explain the absence of observable polarisation in the case of the gas discharge, but it seems surprising that any electricity should pass at all under the circumstances, between the gas and the electrode. My calculations, however, depend on the extension of an experimental formula

up to the electrode, which may not be justifiable; but another explanation is possible. If we ask at what distance from the kathode is the potential 1 volt less than at the kathode itself, we find it to be about the thousandth part of a millimeter. It will constantly happen that particles will approach the kathode from that distance, and the work which has to be done in the transference of the positive ion to the electrode may be partially supplied by the energy acquired in the fall.

Effect of a Magnet on the Negative Discharge. Confirmation of the Theory.

In my former paper I described a method by means of which I hoped to be able to measure the charges carried by the ions, and thus directly to test the truth of the theory. It is clearly most desirable that this should be done, for if it could be shown that the molecular charges are the same as those carried by the atoms in electrolytes, all further doubt as to the correctness of the view which I advocate would vanish. I have met with very considerable difficulties in the attempt to carry out the measurements in a satisfactory manner, and have only hitherto succeeded in fixing somewhat wide limits between which the molecular charges must lie.

According to one theory, particles are projected from the kathode. The observed effect of the magnet on them is exactly what it should be under the circumstances. The path of the particles can be traced by means of the luminosity produced by the molecular impacts. If the trajectory is originally straight, it bends under the influence of a magnet. The curvature of the rays depends on two unknown quantities, the velocity of the particles and the quantity of electricity they carry.

If the particles carrying a charge are moving with velocity at right angles to the lines of force, the radius of curvature r is determined by the equation

$$\frac{mv^2}{r} = \mathrm{M}ve \quad \text{or} \quad \frac{e}{m} = \frac{v}{\mathrm{M}r} \qquad \text{(1.)},$$

where m is the mass of the particle. If the particles originally at rest, start from the kathode at which the potential is taken as zero, and arrive, without loss of energy, at a place where the potential is V, we should have another equation, namely :—

$$2\mathrm{V}e = mv^2 \qquad \text{(2.)}.$$

Eliminating v, we find :—

$$\frac{e}{m} = \frac{2\mathrm{V}}{\mathrm{M}^2r^2} \qquad \text{(3.)}.$$

The quantity e/m thus obtained can be directly compared with the known electro-chemical equivalents. The assumption that in the passage of the particles, the whole work done appears as acceleration can never be perfectly realised, and experiments only can decide how nearly we may approach it. In the dark space surrounding the kathode the dissipation of energy is probably small, and we have every reason to believe that there the velocities are very high. I need not enter here into the many experimental difficulties which I have encountered, and which I hope soon to overcome more completely than I have yet been able to do. In the experiments hitherto carried on equation (2) cannot be assumed to hold. The equation (3) may be used, however, to fix an upper limit for e/m. A lower limit can be calculated as follows:—As long as the effect of the magnet on the particles projected from the kathode shows any directional preponderance, we may take it that the velocities of the particles must be greater than the mean velocity in their normal state. For it is clear that, if distribution of velocities was symmetrical in all directions, the magnet would have equal and opposite effects on the charges which move in opposite directions; and if by mutual impacts the velocity is reduced to its normal value, it will also have lost any directional inequality. We may obtain a lower limit for e/m if in equation (1) we calculate

$$\frac{e}{m} = \frac{v}{\mathrm{M}r} \quad \text{(4.)},$$

by putting for r the smallest radius of curvature which can with certainty be traced in the glow, and for v the mean velocity of the particle, according to the kinetic theory of gases.

In an actual experiment M was 200; r diminished with increasing distance from the kathode. The greatest value which could with certainty be measured was about 1 cm. V was 225 volts at the same place. Taking these numbers, we get for the upper limit

$$\frac{e}{m} < 11 \times 10^5.$$

In the glow the radius of curvature is quickly reduced to about $\frac{1}{2}$ cm., showing that the luminosity is directly due to a conversion of directional into thermal motion. The gas in the actual experiment was nitrogen much contaminated with hydrocarbons. The value of the mean velocity in the state of equilibrium will depend on the supposition we make as to the nature of the particle which carries the charge.

It will be sufficient to consider the cases indicated in the following table:—

Nature of particle.	Velocity of mean square.
Hydrogen atom................	26×10^4
Hydrogen molecule	18×10^4
Nitrogen atom..................	7×10^4
Nitrogen molecule..............	5×10^4

As we are only dealing now with the order of magnitude, the temperature need not be taken into account, and we may take v to be 10^5; we thus obtain

$$\frac{e}{m} > 10^3.$$

The actual value of e/m is 10^4 for hydrogen and $0{\cdot}7 \times 10^3$ for nitrogen, if we imagine each atom of nitrogen to carry the same charge as the atom of hydrogen in water; but, as nitrogen may unite with these atoms of nitrogen we must assume three charges at least to be carried, which would make e/m equal to 2×10^3.

It thus appears that there is nothing in the actual facts which is in any way not in harmony with the theory. The lower limit for e/m comes very near the actually observed values, and it is not astonishing that the upper limit yields so great a value. It will be seen that in equation (3) the radius of curvature enters as the square. I think I may take the experiments hitherto recorded as a confirmation of the theory. Assuming the theory to be correct, they show that in the glow the particles are quickly reduced to a velocity of the same order of magnitude as the mean velocity in an unelectrified medium. If the particles do not carry fixed charges, they must become electrified by contact at the electrodes. This is the view generally taken, and it is interesting to trace its consequences. The change e of a sphere touching a plane charged with surface density σ is* given by

$$e = \tfrac{2}{3}\,\pi^3 a^2\sigma = 20\ a^2\sigma \text{ approximately.}$$

Substituting for σ the highest value which I have obtained in my experiments, about $2{\cdot}5$ electrostatic units, e would be numerically equal to $50a^2$. If for a^2 we take about 5×10^{-10}, which is the molecular range obtained from experiments in gases, we should be above the mark. This, when substituted in the above, gives for e the value 10^{-17} in electrostatic units, instead of 10^{-12}, that is, the charge would be about 100,000 times less than according to our theory. Applying the equation (4), I calculate that, according to the hypothesis of electrification by contact, the average velocity of the molecules would only have been 2 cm. a second, which is a *reductio ad absurdum.*

* Maxwell, vol. 1, p. 257.

Some Questions relating to the Positive Discharge.

There is one theory of electric discharges which, as a scientific curiosity, is of interest. It asserts that a perfect vacuum is a perfect conductor, but that the molecules flying about the vacuum impede the passage of the current. If no discharge actually passes through highly rarefied gas in an experiment, it is, according to this view, because there is a resistance at the surface of the electrodes. It is interesting to speculate what the world would be like if this theory was true. It is perhaps not fair to urge that we should live in perpetual darkness, because the upholders of the theory could not consistently adopt the electro-magnetic theory of light, whose essence is a stress in the medium. But I do not see how we should have any electrostatic effects at all, at any rate in highly exhausted vessels. In order that gold leaves should remain divergent *in vacuo*, it is by no means necessary that no escape of electricity should take place from them. They will collapse, though their surface may be perfectly impermeable to the discharge, as long as currents may flow in the surrounding medium. If the gold leaves are charged positively, negative electricity would flow towards them, cover them, and protect the leaves, so as to prevent any repulsion. There are other fatal objections against the theory which will survive nevertheless, for, like all paradoxes, it has an irresistible attraction to a great many minds.

The following experiment has convinced those to whom I have been able to show it that the discharge consists of a diffusion of charged atoms or molecules. The apparatus used is that by means of which Balfour Stewart and Tait have carried on their researches on the heating of a rotating disc *in vacuo*. In front of an ebonite disc, electrodes were introduced so that the line joining them was parallel to the plane of the disc, viz., one electrode opposite the centre, and the other opposite the edge of the disc. When the latter is rotated, it carries round with it the air in its neighbourhood. If the current consisted of a motion of the medium, the particles of air could not affect the distribution of the lines of flow. But it is found that the discharge is drawn up or down according as the motion of the air is upwards or downwards. The curves formed by the discharge are similar to those observed when a magnet acts on a positive discharge, and their cause is identical, as the magnetic force also acts on the particles, and tends to draw them through the discharge, as I have explained in my previous Bakerian Lecture.

Photographs of the actual appearance have been taken, and will be given in the full account of these experiments. It is very striking to see the discharge steadily and slowly deflected by the rotating disc, and so sensitive is it to slight currents in the air, that the heating of the gas by the discharge and the convection currents formed in

consequence are quite sufficient to cause a decided bending, even without any rotation of the disc.

Magnitude of some of the Quantities involved in the Discharge.

As it is necessary to bear in mind the order of magnitude of some of the quantities involved in the discharge, I may briefly note some of the most important ones.

The most probable value for the charge carried by each atom of hydrogen I find to be 3×10^{-23} electro-magnetic units, more generally, say, $3\kappa \times 10^{-23}$ where κ is a numerical constant.

Question I:—How does the energy acquired by an ion between two impacts compare with the average kinetic energy of a molecule?

Answer:—The ratio e/m for hydrogen is known to be 10^4 approximately. If a particle of mass m carries a charge e, the velocity generated from rest through a range in which the difference of potential is V is

$$\sqrt{\frac{2e\mathrm{V}}{m}} \quad \text{or} \quad 140 \sqrt{\mathrm{V}}.$$

If V is one volt, this would be equal to 14×10^5; a quantity nearly ten times as great as the mean velocity of a hydrogen molecule. In the positive part of the discharge when the velocity of diffusion is uniform, the fall of potential in my experiments was, roughly speaking, about 1 volt per millimeter; the mean free path calculated according to the kinetic theory varied between $\frac{1}{2}$ mm. and $\frac{1}{4}$ mm. Hence, on the average, the velocity generated in the atom by electric forces between two encounters exceeds several times the mean velocity in the stationary state. It will appear that the number of atoms carrying charges is small compared to the total number; so that the actually observed rise in temperature need not be considerable. At each impact the atom must give up, on the average, that proportion of its own velocity which it gains during two encounters; and the above numbers show that the energy communicated is very considerable. Hence the luminosity of the positive discharge. Even if by an impact the ions are thrown back, the electromotive force will, in general, be strong enough to reduce it to rest, and send it forward before the next impact. The path of particles will therefore not be straight, and the velocity of the ions before impact will almost entirely be in the direction in which the force acts.

Question II:—If the molecules, each charged with a quantity of electricity e, approach each other with a velocity equal to the mean speed in a homogeneous gas at ordinary temperature, at what distance from each other will they come to rest?

Answer:—If v is taken as 17×10^4, and $e = 3\kappa\times10^{-23}$, the distance is $2\kappa\times10^{-8}$, which is larger, though not considerably so, than the molecular distance. We conclude that two particles charged with the same kind of electricity will, in general, not approach each other sufficiently near to bring other than electrical forces into play.

Question III:—At what distance is the force between two equally charged atoms equal to the force in a field in which the fall of potential is 1 volt per centimeter?

Answer:—$r = \sqrt{3\kappa} \,.\, 10^{-5}$. If the fall is 1 volt per mm., the distance would be three times as great, or about 5×10^{-5}.

Question IV:—What is the proportion of ions amongst the molecules in the positive part of the discharge?

Answer:—I assume, as a first approximation, the velocity of diffusion to be that of the mean velocity of the gas in the normal state. (One measurement of the magnetic deflection in the glow shows that the velocity of diffusion cannot be much greater, and the answer to Question II shows that it cannot be much smaller.) If S is the cross-section of the tube, C the current, and n the number of ions, we have

$$n\mathrm{S}ev = \mathrm{C}\,;\ \mathrm{N}m = \rho,$$

where N is the number of undecomposed molecules, and ρ the density of the gas. Combining the two equations:

$$\frac{n}{\mathrm{N}} = \frac{m\mathrm{C}}{\mathrm{S}.e.v.\rho}.$$

In a typical example, ρ was 5×10^{-7}; $\mathrm{C} = 3\times10^{-4}$; $\mathrm{S} = 2$; and as $\frac{e}{m} = 10^4$, we find

$$n/\mathrm{N} = \tfrac{1}{5} \times 10^{-6},$$

a small fraction, which is suggestively near the fraction obtained for the proportion of positive ions in close contact with the kathode; the two results being observed from altogether different quantities.

Question V:—What is the average distance between the ions in the positive part of the discharge?

Answer:—With the same data as in Question IV, I find for the average distance approximately 10^{-2}, or $\frac{1}{10}$ mm.

Probable Explanation of the Fall of Potential observed at the Kathode.

In carrying out the investigation of which the experiments described in this paper form a part, I have always attached most

importance to the clearing up of those questions on which the mathematical analysis of the subject will have to be based. From this point of view, nothing is of greater importance than the investigation of the surface conditions which must hold between the gas and the vessel, and between the gas and the electrodes. In a previous paper,* I have shown that at any part of the surface of the gas through which no electricity passes the normal forces must vanish, which is not *a priori* evident, if we consider the gas to possess a certain dielectric strength. The fall of potential at the kathode must depend on the surface conditions which hold there, and the following considerations may help to clear up the question.

Imagine, in the first instance, a gas containing a certain number of charged particles, and enclosed in a vessel kept at zero potential, but having a surface impermeable to electricity. We may not be able to realise these conditions, but we may discuss the problem as an ideal case.

How will the charged particles arrange themselves under the influence of their mutual forces? They will, no doubt, travel outwards towards the surface, but will they cling to the surface, condensing, as it were, to form a layer against the solid surface resembling a liquid more than a gas? Or will they form a gaseous atmosphere, diminishing in density from the surface outwards? Or, finally, will they resemble the state of a liquid in contact with a gas, that is to say, will they be in a state of movable equilibrium, a certain proportion always clinging to the surface of the solid, others flying away until brought back by impacts and electric forces? I do not see how the answer to these questions can be given on the theoretical grounds only, and it seems to me that experiment only can decide it. Nor is it necessary, to my mind, that an atmosphere of positive ions should behave exactly in the same way as an atmosphere of negative ions. The only consistent theory of contact electricity we possess is that worked out by Helmholtz, according to which we must, in calculating the work done in the transfer of electricity through a surface, not only take account of electric but also of electro-chemical forces. There are various ways of expressing the same fact. We may say that there must be a definite attraction between matter and electricity, or we may say that the potential energy of a system contains terms involving both electrical and chemical variables, or, finally, that both chemical and electrical forces are due to stresses in the medium, and that in calculating the forces we must add the displacements and not the energies.

In considering the mutual action between electrified particles at molecular distances, it is quite possible, and even probable, that positive and negative electrification may affect the molecular forces

* 'Roy. Soc. Proc.,' vol. 42, p. 371 (1887).

in different ways. The same holds as regards the action between electrified and non-electrified particles. If the theory of electrolytic convection in gases is true, some hypothesis of this nature is necessary to explain the asymmetry of the discharge. Positive ions, according to the theory, will be delivered at the kathode, either by direct decomposition or by diffusion; negative ions will, in the same way, appear at the anode. If, as we must assume by analogy from liquids, a certain normal force is required to effect the interchange of electricities at the electrode, this will become covered, in the first instance, with the ions until the necessary normal force is obtained But we are face to face with the questions previously raised relating to the distribution of ions against the surface of the kathode. If the conditions are such that positive gaseous ions behave partly, at any rate, as a gas; if, instead of clinging to the electrode, they form an atmosphere round it, the fall of potential at the kathode is explained. The law according to which their density diminishes as the distance from the electrode increases depends on an experimental term, as has been stated, and I have not yet arrived at a satisfactory theoretical foundation for the law; but various suppositions may be made, and if we may imagine the layer of positive ions to behave like a thin liquid film having a definite vapour pressure, we may easily imagine that the falling off will take place very much as it actually does. The large fall of potential at the kathode, according to this view, is not so much due to the amount of work which has to be done to effect the interchange of electricity, but chiefly to the fact that for the same surface density at the kathode the thickness of the polarising layer is greater, which must necessarily increase the fall of potential. Thus, if σ is the surface density, and D the molecular distance, the fall of potential would be $4\pi v^2 D\sigma$, if the ions covered the kathode as in an electrolyte; but, according to the observed law, the potential in the neighbourhood of the kathode is given by

$$V = V_0(1 - e^{-\kappa x}),$$

which gives for the surface density

$$\kappa V_0/4\pi v^2, \text{ so that } V_0 = 4\pi v^2 \sigma/\kappa;$$

but $1/\kappa$ is of the order of magnitude of a millimeter, and this shows how much the fall of potential is increased by the increased thickness of the layer. Comparing the two expressions, we may say that the fall of potential at the kathode would be the same as in an electrolyte if in the latter case the mean distance of the polarising layer from the kathode was $1/\kappa$ instead of the molecular distances. The numerical values for $1/\kappa$ are, on the average, about six or seven times as great as the mean free path.

According to this view we may explain why gases in their sensitive state, like flames, behave differently to positive and negative charges. A positive charge will attract the negative ions, which will arrange themselves on the surface, and the requisite difference of potential will at once establish itself. But if a conductor placed in the flame carries a negative charge, the layer within which the positive ions collect will be deeper, and the potential of the conductor may not be sufficient to complete the layer so as to produce the necessary normal force.

It also appears that, just as minute chemical changes affect the polarisation in the electrolyte, so will all similar changes affect in the same proportion the fall of potential at the kathode. If I am right, we must consider the conditions of impact between the metal and the ions, or between the gas and the ions, to be different according as the ions have a positive or negative charge, and this leads us to the next point which it will be necessary to discuss.

If the law of impact is different between the molecules of the gas and the positive and negative ions respectively, it follows that the rate of diffusion of the two sets of ions will in general be different; let us see whether we can find any experimental evidence which may throw light on this point. I think there is some reason to believe that the negative ions diffuse more rapidly, and we may at once trace one of the consequences of such a difference of diffusion. Looking at the positive part of the discharge, which shows no signs of a bodily electrification anywhere, at any rate when there are no stratifications, a quicker negative diffusion means, just as in the case of the so-called migrations in electrolytes, an accumulation of ions at the positive pole. That is to say, at the anode a certain number of the ions must recombine again to form a neutral molecule. It has already been mentioned that at the kathode we must imagine decompositions to be going on, continuing during the discharge, because we know that the necessary electrical forces are maintained there. If the discharge is steady, then decomposed atoms must unite somewhere, and, as just suggested, the reunion may take place at the anode; or it may already take place in or just beyond the negative glow. The two questions are intimately connected. If the molecules are decomposed in one part of the tube and reunite in another, the ions in between cannot travel at the same rate. What leads me to believe in a quicker diffusion of negative ions is the fact described in my former paper, that in the neighbourhood of a discharge, positive bodies apparently become neutralised more quickly than negative ones. I think there runs throughout the whole set of experiments a general tendency for the negative ions to be drawn more quickly than positive ions towards the oppositely charged bodies. Some observations on flames also point the same way. Gold-

stein found, in some of his experiments, that when an electric current passing through a gas is forced through a narrow opening many of the phenomena seen near the kathode appear on that side of the opening which faces the positive pole. It is also known that, if a current passes through a funnel-shaped opening, the fall of potential required is greater in one direction than in the other. Finally, if the current is discontinuous, and a point on the outside of a glass tube is connected to earth, certain phenomena are seen which have been specially investigated by Messrs. Spottiswoode and Moulton. All these facts I believe to be capable of explanation if we remember that, whenever a current passes between solids of different conductivities, a certain surface electrification is necessary to satisfy the conditions of continuity. Gases do not follow Ohm's law, and there will in all probability be an electrification whenever the cross-section alters. The different behaviour of positive and negative electrification will come into play, and this, together with the different rate of diffusion of different ions, will, I believe, be found sufficient to explain the phenomena.

The effect of ultra-violet light on a negatively electrified body is probably due, as has been pointed out, to a chemical action, but we have further to assume that this action is not set up on a positively charged body. If this view is correct, we shall have to take the law of impacts between the gas and the metal to be modified in such a way that a chemical effect only takes place when the metal is charged negatively.

Stratifications.

It is generally considered that the most important test of any theory of the discharge is to be found in the way in which it can explain stratification. Very little is known about the circumstances which produce stratifications, and they show by their lawless behaviour that they are rather to be considered as irregularities in the discharge than as matters of primary importance.

According to our view, the regular diffusion of ions in the positive part of the discharge can only be maintained by a balance of very delicately-adjusted phenomena. The two kinds of ions diffuse with different velocities; they will tend to recombine together, and will occasionally do so. If so, and if the current does not cease to be steady, we must have as many fresh dissociations as combinations in each part of the tube. It does not seem impossible that there may be several stable ways in which the current may pass. It is possible that, besides the discharge which passes as I have just explained, there may be another in which the tube is divided into a succession of parts in which the decompositions alternately outnumber the

recombinations and *vice versâ*. Such a tube would show phenomena very similar to stratifications. This is only a suggestion to show that the theory may ultimately be found sufficient to cope with this difficulty. At present it seems to me to be an open question whether the stratifications are ever seen in perfectly pure gases.

The Dark Space.

No satisfactory explanation has yet been given of the division of the appearance round the kathode into three parts: the first luminous layer, the dark space, and the glow. The division between the dark space and the glow is often very sharp, and it is necessary to discuss how the rapid change in luminosity can be accounted for. It has been suggested that the extent of the dark space represents the mean free path of the molecules. If particles are projected from the kathode at low pressures, comparatively few will impinge in its immediate neighbourhood, but with increasing distance the number of impacts will increase. It has been pointed out by others that the extent of the dark space is really considerably greater than the mean free path of the molecule, calculated according to the ordinary way. My measurements make it nearly twenty times as great. This, however, is not in itself a fatal objection, for, as we have seen, the mean free path of an ion may be different from that of a molecule moving among others. I cannot, however, reconcile the sharpness of the inner boundary of the glow with the explanation given. If the luminosity only depended on the number of impacts, we should expect the parts adjacent to the electrode to be dark and gradually to increase in intensity outwards. The positive ions approaching the kathode would still further reduce the difference in luminosity. I have endeavoured to ascertain the experimental conditions which determine the shape of the boundary of the dark space. The first supposition tested was, whether the boundary was always an equipotential surface. If so, the velocity of the particle projected from the negative electrode would be the same all over the boundary, and we might imagine that the luminous appearance of the glow depends on some minimum kinetic energy which the impinging particles must possess. The darkness is, however, not limited by an equipotential surface.

A large cylindrical vessel contained two negative electrodes parallel to the axis of the vessel. The anode was formed by a cylindrical wire netting surrounding the kathodes. Under these circumstances the dark space and glow present some peculiarities, which I shall describe on another occasion. The shadow phenomena described by Goldstein are beautifully seen, and can be photographed, and as the sides of the glass vessel do not interfere (as it now appears they did

in Goldstein's original experiments), I have been able to supplement his observations in several details. At present I only wish to state that the edge of the dark space is, under these circumstances, far from being an equipotential surface; so we must look for some other explanation. We can assure ourselves in another way that it is not a certain minimum kinetic energy which determines the boundary of the dark space. In my previous Bakerian Lecture I followed others in the statement that an increase of current diminishes the thickness of the dark space; but this I find is not correct. If the dark space is carefully watched while the current is diminished or increased by altering the resistance of the circuit, it is seen to contract or expand slightly, always being widest when the current is strongest. Such an increase of current is accompanied by an increased fall of potential; the difference of the potential at which the dark space ends can therefore be altered at will.

I can at present only think of one way of accounting for the facts, but wish for the present to express my views on this point with due caution.

From the magnetic experiments it appears that the velocities of the molecules are reduced quickly in the luminous glow, but not at any rate to the same extent in the dark space. If that is the case, there must be some change in the law of impacts, as we pass from the dark space into the glow; and the simplest supposition to make seems to me to be that the strength of the electric field is an important factor in the transformation of energy which takes place in the collisions. We may imagine that if the electric field is sufficiently strong, the ion will not lose much of its energy during impact, but that in a weak field the velocities are reduced at a much quicker rate. It is clear that if the molecular forces are strong, two molecules must approach much more closely together before their mutual action comes into play, and therefore what must be considered an impact must happen more seldom in a strong than in a weak field. Near the edge of the dark space the electric forces are found to diminish very rapidly, and it is a question worth investigating whether the edge of the dark space is a surface at which the electric force has some constant critical value. I know of no facts which are against this view, and my experiments have hitherto all been consistent with it. The slight widening of the dark space by increase of current would at once be explained if my hypothesis is correct. In the above-mentioned case of two parallel kathodes, there is always a luminous layer in the equidistant plane between them, even when the width of the dark space due to one kathode alone extends sufficiently far to include the other. In this plane the fall of potential is easily seen to be small, and the shape which the dark space assumes seems to me to agree very well with the supposition that there is a critical rate of

potential which determines the edge of glow. But this point must be left to be settled by future experiments.

As regards the inner luminous layer closely adjacent to the negative electrode, it seems due to the positive ions approaching the kathode, and not, like the glow, to the negative ions projected away from it. This is shown by the fact that a wire placed inside this layer casts a shadow *towards* the kathode, and also by the distortion it experiences in a magnetic field. It is remarkable that this luminosity, due to the impact of positive ions, shows according to Goldstein, at any rate in the case of nitrogen, the spectrum of the positive part of the discharge.

Conclusion.

We may now in conclusion shortly summarise the results arrived at. A gas in its normal state contains no free ions, but, if through any chemical or physical causes the molecules are broken up in an electric field, ions form, and the gas becomes a conductor. Supposing the difference of potential of two electrodes is gradually increased, a point will be reached at which a spark will pass, that is to say, the molecules will be broken up by electric forces, the positive ions diffusing towards the kathode will tend to form a polarising layer of finite thickness, increasing in width as the pressure diminishes. If the discharge becomes steady, the decompositions are continuously kept up at the kathode, the negative ions being projected with great velocity away from it. These ions will move through the so-called dark space without much loss of energy by impacts, but when, probably owing to sufficient diminution in the electric force, the impacts become more frequent, the translational energy becomes transformed into the luminous vibrations of the glow. The positive ions forming an atmosphere round the kathode must have a greater velocity the nearer the kathode, where their energy becomes visible in the first luminous layer. Whether decompositions take place only at the electrode or through a finite distance from it is at present an open question, nor can we decide as yet whether the negative molecules projected outwards are the main carriers of the current inside the dark space. In the dark space the negative ions will accumulate and meet the positive ions proceeding from the positive part of the discharge. We shall expect at some point towards the outside of the glow the free ions to become more numerous than in other parts of the discharge. Here we find a small fall of potential and no luminosity; this is the dark interval separating the positive part of the discharge from the negative glow. A number of ions probably reunite in this part to form molecules, and in case it should ultimately be found that positive and negative ions diffuse with the same velocity, we should have to conclude that as many molecules as are

decomposed at the kathode recombine in this dark interval. If, as seems more probable to me, it should be found that the negative ions diffuse more rapidly, the recombination will in part take place at the anode. If the conditions in the tube are such that the gas may divide into layers, such that in alternate strata the decompositions outnumber the recombinations, and *vice versâ*, stratifications will form.

Such is the general outline of the theory, which may have to be modified in detail, but which, I believe, has a strong element of truth in it.

The possibility of a volume electrification is denied by some of Maxwell's disciples, who look on a current of electricity as on a flow of an incompressible liquid in a closed circuit. But there is nothing, as far as I can see, in the conclusions I have drawn from the gas discharges which is inconsistent with the fundamental tenets of Maxwell's theory, however much they may disagree with the accessory embellishments with which that theory is occasionally adorned. There may be a volume electrification without interfering with the equation of continuity of an incompressible liquid as long as we admit the possibility of displacement currents and displacements in conductors, and I see nothing improbable in this. The ordinary equations for the currents in a non-homogeneous solid (or any solid if inequalities of temperature are taken into account) give a volume electrification which can only be destroyed by the introduction of a quantity which is analogous to hydrostatic pressure, and the sole purpose of which is to destroy all electrifications except at the surface of bodies. We know of no physical phenomena which can justify the introduction of such a quantity, which seems to me unnecessary. The existence of a volume electrification can be shown to exist when a current passes from one liquid to another floating on its surface. Chemical effects are observed in the region in which the liquids begin to mix, and these can be explained by the electrification which accompanies each change in electric conductivity. Maxwell's equations assume conductors to be homogeneous throughout; whenever we are dealing with average effects only, this assumption is justified. We deduce, in a similar way, the equations which represent the transmission of light by assuming that each transparent body is replaced by a homogeneous medium having certain properties. But although this simplification is allowable in discussing some of the phenomena, there are others in which it becomes necessary to go a step further, and, considering the structural constitution of the body, to take into account separately the effects of the medium separating the atoms, and the effects of the atoms themselves. In all branches of physics we are gradually forced by the advance of knowledge to abandon the assumption of homogeneousness, and if that is done, no further difficulty stands in the way of bodily electrifications; for

we may take them to be really only surface electrifications between the atoms and the medium.

I have offended in another manner against so-called modern views of electricity, for I have spoken of positive and negative electricity as real substances possessing a separate existence. I have tried to place myself, however, under the shelter of recognised authority by quoting at the top of this lecture Helmholtz's saying that we have as much ground for the supposition that electricity has an atomic constitution as we have for the atomic constitution of matter. We must trust to the future to bring this view into harmony with the electromagnetic theory of light, which may be accepted now as an established fact. There is no real antagonism between the two views. If ever we are able to explain chemical and gravitational attraction by the stresses in a medium, we shall still find it convenient to speak of atoms and molecules; and in the same way the belief in an electric strain and stress is consistent with a belief in something in the atom from which the strain proceeds, and which may be taken as the elementary quantity of electricity. Even taking the extreme view that electric stress is due to vortex filaments in the ether, we need only assume all these filaments to have the same intensity, and some to end at the surface of atoms, in order to reconcile apparently antagonistic views. But there is no need to commit ourselves at present to any particular ideas. In some electric phenomena we shall find it most convenient to speak of electric strain and stress (displacement I think to be a misleading term, which, however, has come too much into use to be dispensed with); in other, and at present more numerous, cases, we shall still continue the old nomenclature, and speak of positive and negative electricity as real quantities. The subject of electro-chemistry is one of primary importance in the present state of science. The different behaviour of positively and negatively electrified particles points, as I have tried to explain, to an unsymmetrical modification of molecular forces by electrification. It is not sufficient to add geometrically the effects of molecular and electrical action, but it is necessary to take account of the interference between chemical and electrical forces. The exact nature of this interference must partly be solved by chemical investigation, but the discharges of electricity through gases still promise a rich harvest to the investigator.

Appendix.

"The Discharge of Electricity from Glowing Metals." By ARTHUR STANTON, B.A.

It has long been known that certain bodies undergoing chemical decomposition are capable of discharging electricity through the

surrounding air. A few desultory experiments were made in Dr. Schuster's laboratory, during the hot days of summer about two years ago, as to this discharge when the body was decomposed in the focus of a large concave mirror. The method, depending on exceptionally brilliant weather, is necessarily inconvenient in this country. Subsequently, during the Long Vacation, I made experiments in the same direction, and tried to use a piece of hot metal for the supply of heat to effect dissociation. These attempts led to an observation of the conditions under which a hot electrified piece of copper or iron could retard a charge of electricity at a good red heat. I observed in my first experiments that if a copper soldering bolt, heated to full redness by a gas blowpipe, was placed on an insulating stand and negatively electrified, discharge took place very rapidly, and occurred so long as the bolt remained visibly red; that if the bolt was repeatedly heated in an oxidising flame, and electrified, discharge became continuously slower, and that ultimately the copper was capable of retaining a charge perfectly at a full red heat. If the copper bolt, being in the state last described, was allowed to cool completely, the oxide of copper chipped off, and the metal, on heating to redness, behaved generally as at the commencement of the experiments.

An iron bar was found to behave similarly.

The experiment was afterwards repeated, with the substitution of a wire kept hot by a current for the massive bar of metal.

In the later form of the experiment, the hot body was connected to earth, and the discharge of an electrified conductor in the neighbourhood observed. The wire was wound upon a mica frame with thick copper terminals, which served to give the frame rigidity; the other electrode of the system consisted of a clean flat copper plate, at a distance of 2 or 3 cm. from the frame. The wire used varied from 0·3 to 0·5 mm. in diameter, the length being about 60 cm. Both the frame and the flat plate contiguous to it were enclosed in a glass or cylinder surrounded by water to keep it cool, and provided with tubes for the introduction of gases.

The following are the results obtained:—

First, if the conductor contiguous to the wire be positively electrified.

Here the clean copper wire on becoming red-hot rapidly discharges the conductor; when a uniform film of oxide is formed, the discharge ceases.

If the containing vessel be now filled with hydrogen, and the wire again heated, a similar discharge takes place until the oxide film is completely reduced; the conductor thereafter retains its charge perfectly.

Secondly, if the conductor be negatively electrified.

In this case, the copper wire must be heated for a longer time in air before it ceases to effect the discharge, and there is not the same sharpness of definition between the two states. The phenomena observed on heating the now oxidised wire in hydrogen differ also in this case; the hot wire not only effects discharge during the reduction of its oxide coat, but continues to possess this power certainly for a long time to the same degree. Hence, a red-hot copper wire in hydrogen exhibits the curious property of retaining perfectly a charge of negative electricity, and discharging instantly a positive electrification. The hydrogen used was fairly dry, and free from any sensible taste or smell.

In dry nitrogen, the results were similar to those obtained with hydrogen. The gas was carefully dried, and passed for eight or nine hours over the copper before the electrical tests were made; the red-hot copper wire retained a negative charge, but not a positive one.

The above experiments were all made in Dr. Schuster's laboratory, and, in fact, under his immediate supervision. Dr. Schuster has suggested that they are of sufficient interest to publish in their present state, because they show more clearly than the experiments with platinum the nature of the chemical action. In the case of platinum, there is, of course, the advantage that the metal remains generally in the same state, but it is much lessened by the very marked and complex effects of surface condensation and occlusion.

The potential of the bodies used was observed by means of an ordinary gold leaf electroscope, and was such as to cause a large divergence of the leaves. In all cases where discharge took place, it was found easy to cause complete collapse of the leaves.

It is proposed to supplement this paper with one dealing with the phenomena in pure and perfectly dry gas.

OBITUARY NOTICES OF FELLOWS DECEASED.

ROBERT HUNT was born at Devonport, on the 6th of September, 1807, six months after the death of his father, an officer of the Royal Navy, who, together with all the crew of his vessel, was drowned in the Grecian Archipelago. His early education, received partly in his native town, and partly at Penzance, was brief and inadequate, for when only twelve and a half years of age, he was sent to London and placed with a surgeon in practice there. His medical career appears to have been by no means a happy one. He soon contrived, however, to acquire enough of Latin to qualify him for dispensing prescriptions; he gained some knowledge of anatomy by attending Brooke's lectures; and amid the exacting labours of a Fleet Street dispensary he found occasional leisure hours, that enabled him to profit by the use of a good library to which he had been allowed access. For somewhere about eleven years he continued as a druggist's assistant, until at last an illness made it needful for him to return for a season to Cornwall. About this time his grandfather's death put him in possession of a small property on the banks of the River Fowey. With characteristic energy and enthusiasm, he was no sooner master of this source of income than he sold some of the fine old elm-trees on his ground, and with the proceeds kept himself for some months, during which he tramped all over Cornwall, looking at the scenery and the rocks, but more especially mingling with the peasantry, and gathering from their lips the legends and superstitions that still lingered in that remote western county. Long afterwards this early journey bore fruit in his volume on the "Romances and Drolls of Devon and Cornwall," which went through three editions. Hunt's mind showed from his boyhood a markedly poetic vein. While still a young man he published by subscription at Penzance his first literary venture, which was a descriptive poem, entitled "The Mount's Bay," followed in later life by his volume "The Poetry of Science," and "Panthea, the Spirit of Nature." He interested himself in the formation of a mechanics' institute at Penzance, and himself gave the first of its lectures.

Hunt's first distinct entry into the domain of scientific research was suggested to him by Daguerre's experiments in the infant art of photography. He had already gained some practical acquaintance with chemistry and chemical methods of enquiry, so that he was in some measure prepared to begin an independent investigation in photochemistry. His first paper (on "Tritiodide of Mercury") appeared

in the 'Philosophical Magazine' for 1838. From that date onward for some years he continued his enquiries, and published in the Reports of the British Association or elsewhere his results, some of which were of such practical value as materially to conduce to the advance of photography, and to entitle him to a place among the pioneers in this important practical application of science. He still further increased his reputation by publishing his well known manual on photography, which passed through six editions, and by his "Researches on Light," the first edition of which appeared in 1844.

In 1840 Hunt's devotion to science and his indefatigable industry were recognised by his appointment as Secretary of the Royal Cornwall Polytechnic Society, at Falmouth. Five years later, after he had given still further proofs of his abilities, and had attracted the notice of De la Beche, the Director-General of the Geological Survey, he was made Keeper of Mining Records, a new office then created in connexion with the Office of the Survey, at Craig's Court. In 1851, when the School of Mines was started, in Jermyn Street, he became Lecturer on Mechanical Science, retaining at the same time his other appointment in the Mining Record Office. But after a few years he was relieved of the duties of his lecturership, that he might devote himself more uninterruptedly to the laborious duties of the collection and tabulation of the statistics of mines all over the United Kingdom. It was in these duties that he was chiefly engaged during the rest of his long and active life, until, at the age of seventy-six years, he retired on a well-merited pension. While assiduously devoted to the official work of his office, which involved him in continual correspondence with mining authorities and frequent journeys into the various mining districts, Hunt yet found time, mostly in his evenings, to undertake much independent literary work. Chief among these labours was his series of successive editions of Ure's "Dictionary of Arts, Manufactures, and Mines." But he was also a constant contributor to literary and scientific journals. Most of his original scientific work, which was mainly in the department of photographic chemistry, was done between the years 1838 and 1853. In 1854 he was elected into this Society. He died after a brief illness on 17th October, 1887. His remarkably gentle and sympathetic nature led to his making a large circle of friends, and gave him probably a wider influence in the mining community of this country than was possessed by any other man.

A. G.

ROBERT CORNELIS, LORD NAPIER OF MAGDALA, was born in Ceylon on the 6th December, 1810, and died in London on the 14th January, 1890. His father was Captain C. F. Napier, R.A. He was educated

at the East India Company's Military College at Addiscombe, and, after passing out with great credit, on the 15th December, 1826, was appointed to the Bengal Engineers. After a practical course of engineering at Chatham, he proceeded, in 1828, to India, and was soon sent to assist Captain (afterwards Colonel Sir Proby) Cautley in superintending the restored Eastern Jumna Canal, which had fallen into disuse during the decline of the Mogul Empire. In 1840, after a visit to England, he was appointed Executive Engineer at Darjiling, and, in the following year, he was transferred in the same capacity to the Karnál Division of the Public Works. Three years later he was employed in building a new station at Ambala to replace the cantonment of Karnál.

Captain Napier first saw active service in the Sutlej campaign of 1845–46. At Múdkí and Ferozshah, where he was severely wounded, he was on the Staff of Sir Hugh Gough; at Sobraon he was Brigade-Major of Engineers, and at the siege of the hill fort of Kangra he was Chief Engineer. For his services in the campaign he was thanked by Government, and received the brevet of major. In 1848–49 he served in the Panjab campaign, and was present at the siege of Multan, where he was severely wounded; the battle of Gujrat; and with Sir Walter Gilbert during the memorable pursuit of the defeated Sikh troops.

In 1849 Colonel Napier was appointed "Civil Engineer for the Panjab," and whilst holding that post displayed great boldness and capacity as an engineer. Roads, such as the trunk-road from Lahore to Peshawar, were made; great rivers were bridged; old irrigation canals were reopened, and new ones, such as the Bári Doab Canal, were projected; civil buildings were erected; and trees were planted along the canals, or in large plantations, for fuel. In 1852-53 Colonel Napier was employed in the Black Mountain campaign against the Hassanzai tribe, and in the expedition against the Bori Afridi tribes; and in 1857, after a visit to England, he was appointed Officiating Chief Engineer of Bengal. His services were, however, soon required on another field; throughout the operations conducted by General Havelock for the relief of Lucknow he served as Chief of the Staff to General Outram, and during the second defence of the Residency he directed the engineering operations. Though wounded at the relief of Lucknow, he was able to take part in the siege and capture of that town; and afterwards, during the campaign in Central India, he commanded a brigade, and fought the action of Jaura-Alipore. For his services during the Indian Mutiny he received the thanks of Parliament, and was made a C.B. and K.C.B.

During the China war of 1860, Sir Robert Napier commanded a division under Sir Hope Grant; and at its close he received the thanks of Parliament, and was made a Major-General "for distin-

guished military services." On his return to India he was appointed to the command of a division in Bengal, and nominated a member of the Council of the Governor-General. In 1865 he was appointed Commander-in-Chief in Bombay; and two years later he was selected to command the expedition to Abyssinia. The Abyssinian was one of the most skilfully conducted of military expeditions, and it achieved its object almost without loss. On its termination Sir Robert Napier received the thanks of Parliament; was raised to the Peerage with the title of Lord Napier of Magdala; was made a G.C.B.; and was presented by the Corporation of London with the Freedom of the City, and a sword of the value of £200. Lord Napier was afterwards Commander-in-Chief in India (1870–76); Governor of Gibraltar (1876–82); and Constable of the Tower (1887-90). He was made a Field Marshal in 1882, and was a G.C.S.I.; he was also an Honorary D.C.L. of Oxford, and was elected a Fellow of the Royal Society in 1869.

Lord Napier never wrote anything except his official reports and despatches, and never spoke in public except upon subjects of which he was master. His speeches were always to the point, and they were listened to with attention. He was a man of perfect courage, exquisite modesty, and great simplicity of character, who was animated by a lofty conception of duty, and never, in word or deed, departed from the high ideal at which he aimed. A distinguished soldier, a sage counsellor, and a loyal servant of the Queen, he won the complete devotion and implicit confidence of the European and native soldiers who served under his orders, and throughout his long life he employed his great talents in promoting the best interests of the Empire he loved so well.

C. W. W.

Dr. Cobbold was educated at Charterhouse, and matriculated at the University of Edinburgh in 1847 as a student of medicine, having previously served a three years' apprenticeship with Mr. Crosse, of Norwich, one of the most eminent surgeons of his time. Being already possessed of great skill and dexterity in dissection and in the making of museum preparations, he became, in his second year of medical study, prosector to Professor Goodsir, and was thus led to abandon practical medicine for anatomy. He graduated in 1852, and was soon after appointed Curator of the Anatomical Museum, and began to lecture on comparative osteology in the museum.

In 1856 Dr. Cobbold removed to London, and thereafter devoted himself chiefly to the study of animal parasites. In 1864 his well-known systematic work ('Entozoa: an introduction to the study of Helminthology,' London, 1864) appeared, to which he added in 1869 a supplement containing his later researches.

Dr. Cobbold's most important original contributions to Helminthology were his experimental researches on *Tænia mediocanellata* and other Cestodes, published in the work just referred to, his observations on *Distoma hæmatobium* (*Bilharzia hæmatobia*, 'Brit. Med. Journ.,' 1872), and those relating to the so-called *Filaria sanguinis hominis* (*F. Bancrofti*) published in the Journal of the Linnean Society in 1878.

Dr. Cobbold became a Fellow of the Royal Society in 1864. He lectured on Zoology at the Middlesex Hospital from 1860 to 1873, was Swiney Professor of Geology from 1868 to 1872, and subsequently Professor of Helminthology at the Royal Veterinary College. His last communication to the Linnean Society was read on March 4, 1886, not many weeks before his lamented and unexpected death.

J. B. S.

John Ball, Honorary Fellow of Christ's College, Cambridge, was born in Dublin, August 20, 1818, being the son of the Right Honourable Nicholas Ball, M.P., formerly Attorney-General for Ireland, and latterly Judge of the Irish County Common Pleas. At a very early age he developed a love of both physical and biological science, which was encouraged by his father, and greatly stimulated when he was yet a child by visits to the Continent, and especially to the Alps, during which he collected assiduously, and in his first decade taught himself to measure heights barometrically. At thirteen, his family being Roman Catholic, he was sent for three years to the College of St. Mary's, Oscote, now Stonyhurst, where he received a classical and mathematical education. This was at a time when (not as now) scientific proclivities amongst the students were repressed rather than developed, and where his principal amusement, chemistry, was (as he has himself recorded) pursued under every discouragement.

It was at the meeting of the British Association at Dublin in 1835 that Ball's scientific tastes first met recognition. After a week's thorough enjoyment of nearly all the sections, he was placed by his father in charge of the present Professor of Botany at Cambridge (Mr. Babington), and R. M. Lingwood, in order that he might accompany these gentlemen on a scientific tour which they were about to undertake in the West of Ireland. An account of this tour appeared in the 'Magazine of Natural History,' vol. 9, 1836, p. 119, wherein the geological observations were supplied by young Ball.

In 1836 Mr. Ball was sent to Christ's College, Cambridge, where he devoted himself chiefly to mathematics, and in 1838 contributed a paper to the Mathematical Section of the British Association which was favourably noticed by Sir William Hamilton. At Cambridge he renewed his friendship with Mr. Babington, and made that

of the Rev. J. S. Henslow, then Professor of Botany to the University, whom he accompanied on his botanical excursions with his pupils; and to the influence of these two botanists is to be attributed the devotion of the chief part of Ball's scientific life to botany. In 1839 he came out twenty-seventh Wrangler in the Mathematical Tripos, but his religion debarred his receiving a degree, as it had already frustrated all hopes of a scholarship or fellowship.

In 1845, after being called to the Irish Bar, wherein he never practised, he revisited the Alps, and undertook a series of observations on the Glaciers near Zermatt; these he never published, regarding them as only confirmatory of those previously obtained by Professor James Forbes.

Mr. Ball's next occupation was official. In 1846 he was appointed an Assistant Poor Law Commissioner for Ireland, in view of the distress caused by the ravages of the potato disease. The strain of this work on his mind and body was so great that after a year of arduous labour he was obliged to resign and seek abroad a restoration of health. After two years he returned to the office with the appointment of Second Commissioner, which he held till he entered Parliament as Member for Carlow. In 1855 he became Under Secretary for the Colonies, a post which he held for two years, proving himself a most efficient and energetic official, and one having the interests of science always at heart. Amongst other services to science may be especially mentioned the organising the Palliser Expedition, to ascertain the positions of practicable routes across the Rocky Mountains of British North America. To this expedition he had appointed as geologist and naturalist (with a staff of collectors), Dr. now Sir James Hector, F.R.S., the present Director of Geological and Meteorological Surveys in New Zealand. The principal results were the survey of four passes, including that of the "Kicking Horse," now crossed by the Canadian Pacific Railway, and the first knowledge ever obtained of the geology of the vast regions of West Canada and the Rocky Mountains. Mr. Ball also took an active part in urging upon the Colonial Governments the importance of issuing inexpensive floras of the British Colonies, which had been initiated by the late Sir W. Hooker.

In 1858 Mr. Ball's Parliamentary career ended with the expiration of the Ministry, but happily there did not depart with it his influence with future Governments; for he had meanwhile formed political friendships that lasted during his lifetime, and of which he freely availed himself on many occasions in the interests of science.

In 1858 he stood for Limerick, but was defeated. In the words of an obituary notice in the 'Times,' "A cloud was then rising in the horizon, which gravely disturbed the Catholic constituencies, though the rest of the world knew little of it as yet. This was the Italian

question. The Irish priests foresaw the coming struggle, and demanded that their candidates should take the side of the Papacy and the Duchies against Piedmont and the Revolution. This John Ball, though a good Catholic, refused to do, and he was therefore opposed by the Irish priests, and, after a hard struggle, he was defeated."

During the remainder of his life Mr. Ball devoted himself to science as an accomplished and enthusiastic amateur, untrammelled by professional duties, and with a sufficient private income to gratify his love of travel, and of both physics and botany. He was further an excellent linguist, gifted with an uncommonly retentive and accurate memory, an experienced mountaineer, and he had, through his connections and his early visits to the Continent, scientific friends in most of the great capitals. Though by far the greater part of his time and energies was devoted to the Alps, he made extensive journeys in Hungary, Italy, Sicily, Spain and Portugal, Morocco, Algeria, Tunis, the Canary Islands, and the United States, and a cursory visit to the West Coast of South America and Brazil, everywhere collecting and observing, and forming friendships with scientific men that were kept up by an indefatigable correspondence.

Of Mr. Ball's scientific works, the most extensive were the Alpine Guides. It has been well said of them by a most competent authority that "In the history of guide books the 'Alpine Guide' stands where 'Dr. Johnson's Dictionary' stands amongst dictionaries." The basis of this work is of course topographical, but the geological and botanical features of every subdivided area of the great chain of the Alps are dwelt upon with such intelligence and accuracy that no scientific man would regard his outfit for an Alpine tour as complete without Ball's guides. He also wrote the greater part of the 'Journal of a Tour in Marocco and the Great Atlas,' being the records of an expedition made to that country in 1871, in company with Sir J. Hooker and Mr. G. Maw.

The 'Spicilegium Floræ Maroccanæ' is the work by which Mr. Ball will be best known to botanists. It is a virgin flora, the first ever attempted of the country, which, and especially its mountain regions, was in fact botanically as well as geographically previously unvisited; and the materials were almost exclusively those formed by the members of the expedition. These Mr. Ball worked up with scrupulous care, and by the light of his exact knowledge of the kindred floras of Spain and the Southern Alps, with results that are beyond criticism. In a botanico-geographical point of view the unexpected conclusion was arrived at, that the Marocco flora is European, not sharing (as it was expected to do) in the peculiarities of the Canarian and Madeiran, thus confirming the great antiquity of the latter.

His other principal botanical papers are "On the Origin of the Flora of the European Alps," read before the Royal Geographical Society, and a discussion on the origin of the South American flora, published in his 'Notes of a Naturalist.'

Mr. Ball's 'Notes of a Naturalist in South America,' is a work unique of its kind. It embraces the observations and reflections on various scientific subjects that he made, or that suggested themselves to him, during a five months' voyage extending over 18,400 miles of ocean and embracing 100° of latitude, during which he passed only seventy days on land. The route was from England *viâ* the West Indies to Panama, thence down the West Coast of South America to Chili, through the Straits of Magellan to the Plate river, and to Brazil, and so home. At whatever point he landed, or even touched, he was quick to secure a trip to the mountains or forests, bringing back collections and notes of value and interest; and the result is a work of which it has justly been pronounced to be worthy of a corner on the same shelf as those of Darwin, Walker, and Bates. In fact, no other narrative gives consecutively a view in accurate outline of the geographical, meteorological, and botanical features, absolute and comparative, of the different countries along the West Coast of South America from Panama to Fuegia. An appendix contains a description "of the fall of temperature in ascending heights above the sea level," and "Remarks on Mr. Croll's Theory of Secular Changes of the Earth's Climate." The botanical results are embodied in a paper read since his death before the Linnean Society of London.

In meteorology, besides the appendix to the 'Notes of a Naturalist,' mentioned above, he contributed papers "On Thermometric Observations in the Alps," and "On the Determination of Heights by means of the Barometer," to the 'Reports of the British Association;' and he was the first to suggest the utilisation of the electric telegraph for meteorological purposes connected with storm warnings, in a paper "On Rendering the Electric Telegraph Subservient to Meteorological Research," read before the British Association in 1848. This suggestion was not carried into effect till 1861.

On the subject of glaciers, he published in the Geological Society's Journal a notice of "The former existence of small Glaciers in the County of Kerry," and in the 'Philosophical Magazine,' papers on the Structure of Glaciers, on the cause of their Descent, and "On the Formation of Alpine Valleys and Alpine Lakes."

Personally Mr. Ball was one of the most agreeable of men, of an affectionate nature, of warm sympathies, simple minded, and generous in thought and action. His company was much sought, from the fund of information he possessed, and the charm of his manner in communicating it. His services to science were perfectly disinterested, and his aid never withheld. His position in society was a rare one,

counting as he did amongst his warmest friends so many of the *élite* of the literary, artistic, scientific, political, and even musical world in England and on the Continent. He was as fond of society as society was of him, and he confided to a friend his belief that to this must be laid the blame of his not having done more scientific work. He was twice married: in 1856 to an Italian lady, Eliza Parolini, daughter of a distinguished naturalist and Oriental traveller, Count Alberto Parolini, by whom he had two children (sons), who survive him, and through whom he came into estates at Bassano, in Venetia. His second wife was Miss Julia O'Beirne, daughter of F. O'Beirne, Esq., of Co. Leitrim, who survives him. He was a Fellow of the Royal Irish Academy, of the Linnean, Antiquarian, and Royal Geographical Societies, and was elected a Fellow of this Society in 1868. Shortly before his death he received the Honorary Fellowship of his Cambridge College (Christ's), a distinction the more appreciated as he had been debarred by his religion from University honours, which he assuredly would have otherwise won half a century earlier.

For the last few years of his life Mr. Ball suffered much from an affection of the throat, which obliged him to pass the winters abroad; and whilst in the Engadine an internal tumour was developed, for which, on his return to England in the summer of 1889, he underwent a severe operation. Under the effects of this he succumbed on October 21, 1889, at his house in South Kensington.

His extensive herbarium and botanical library were left by bequest to Sir J. D. Hooker, the Director of the Royal Gardens, Kew, and the President of this Society, to be dealt with as they should think fit, with the sole object of promoting the knowledge of natural science.

J. D. H.

The Rev. Miles Joseph Berkeley, M.A., F.L.S., born at Biggin Hall near Oundle, April 1, 1803, was the second son of Charles Berkeley, Esq. and his wife, the latter a sister of P. G. Munn, the well-known water-colour artist. His family belonged to the Spetchley branch of the Berkeleys, and had for several generations been resident in Northamptonshire. From the Grammar School at Oundle, he was sent to Rugby, and in 1821 was entered at Christ's College, Cambridge, where he graduated as fifth Senior Optime in 1825. He has left it on record that he became attached to natural history at a very early period, and that his scientific tendencies, both zoological and botanical, which had been fostered at Rugby, were further stimulated at Cambridge by an intimate acquaintance with the late Professor Henslow. His first clerical duty was the curacy of Thornhaugh, in Northamptonshire, where he was ordained in 1827; and in 1830 he became curate of St. John's, Margate, from which time for upwards

of sixty years his labours and writings as a botanist, and especially as a mycologist, were continuous.

The Mollusca were, however, the first objects of Berkeley's study. As a boy he had made a large conchological collection, and had turned his attention to the structure and habits of the animals of British species. His earliest scientific paper was "On new species of *Modiola* and *Serpula*," published in the 'Zoological Journal,' for 1828. It was followed by "On the internal structure of *Helicolimax Lamarckii*;" "On *Dentalium subulatum*;" "On the Animals of *Voluta* and *Assiminia*;" all in the same journal (1832–1834); and "On British Serpulæ," and "*Dreissena polymorpha*," in the 'Magazine of Natural History' (1834–6).

At Margate Berkeley's attention was naturally directed to the study of marine Algæ, and in 1833 he brought out his 'Gleanings of British Algæ,' a work devoted to the more obscure and little known species.

In 1836, at the request of the late Sir W. Hooker, Mr. Berkeley undertook the formidable task of systematizing the British Fungi for that author's 'British Flora.' This was a work of great research and labour, which had never before been attempted with any approach to completion. The "Systema Fungorum" of the illustrious Swedish mycologist, Professor Fries, was adopted in it, and carried out with many additions and improvements. In 1857 appeared his 'Introduction to Cryptogamic Botany,' which remained the only standard work of the kind in the English language till the publication last year of Bennett and Murray's 'Handbook.' It was followed in 1860 by his 'Outlines of British Fungology,' and in 1863 by the 'Handbook of British Mosses.'

In 1846 Mr. Berkeley commenced his study of the potato murrain, then ravaging our crops. The result was the first complete account of its cause, the *Peronospora infestans*, of which he traced the life history, with the result of demonstrating that the ravages of the disease may be greatly mitigated by early planting and harvesting. In 1847 he undertook a similar investigation of the grape mildew, which he named *Oidium Tuckeri*. These were followed by researches on the fungoid diseases of the wheat, cabbage, coffee, hop, pear, and onion, terminating with one on the tomato disease, which appeared in 1884. It is not too much to say that this country and our colonies are largely indebted to Mr. Berkeley's researches and recommendations for the successful cultivation of their crops.

The kindred subject of vegetable pathology next engaged Berkeley's attention. It was virgin soil, and a series of admirable papers on the subject which appeared in the 'Gardener's Chronicle' between 1854 and 1857 are the foundations of our knowledge of this most difficult and important subject. They were followed by

the article 'On the Diseases of Plants' which he contributed to the 'Cyclopædia of Agriculture.' Then a multitude of articles on kindred subjects, on botany and horticulture, appeared from time to time in the 'Gardener's Chronicle' between 1840 and 1880. Unfortunately they are not recorded in our 'Catalogue of Scientific Papers;' where, however, Berkeley is credited with upwards of a hundred articles in other periodicals, up to the year 1873, since which time twenty-one more are on record.

Of Berkeley's contributions to mycology it is impossible here even to enumerate the more important. By himself, and in some instances in conjunction with his friend C. E. Broome, Esq., F.L.S., of Batheaston, and of M. A. Curtis, for North American Fungi, he published some 6,000 species, including many new genera, from all parts of the world, arctic, antarctic, temperate, and tropical. Of these there are preserved in his own herbarium 4,886 species, that is, nearly half of the whole number that the herbarium contained. In 1879 he unconditionally gave his mycological herbarium to Kew, and whether for its extent, its extraördinary richness in types of genera and species, the number of analyses made by himself with which it is enriched, or the extent to which it illustrates the life history of so many of the great pests of agriculture and horticulture, it is unquestionably unique of its kind. But unprecedented as even his contributions to systematic mycology are in importance, they are far surpassed by the fact, that he was the originator of the study in this country of the life history of fungi, and thus contributed largely to the development of our knowledge of the biological problems now known to depend for their solution on a profound study of the lowest Orders of plants.

Mr. Berkeley was a man of great refinement, an excellent classical scholar, an accomplished man of letters, and an exemplary pastor. In person he was tall and portly, with a noble head, and he was singularly genial in manner. There is an excellent portrait of him in the rooms of the Linnean Society. As may well be supposed, he was one of the most hard working of men. For many years of his life he eked out his most scanty clerical income by keeping a school of some twenty or thirty boys, who, being boarders, left him only the very early morning for his botanical work, which was regularly commenced at 4 A.M. His life history would not be complete without a further allusion to his spiritual duties. From Margate he was in 1838 beneficed to the perpetual curacy of Apethorpe and Woodnewton, Northamptonshire, the emoluments of which never exceeded £180 per annum. Meanwhile, however, his merits had attracted the notice of the late Dr. Jeune, Bishop of Peterborough, who had said, that if ever he rose to the bench the first suitable living in his gift should be bestowed on Mr. Berkeley. This did not occur till 1868, when

the Bishop, true to his promise, presented him to the Vicarage of Sibbertoft, in Northamptonshire, then worth about £400, but which rapidly dwindled as agricultural distress supervened.

Of private property he had none, and he himself educated his fifteen children, thirteen of whom lived to be over twenty-one years of age. His works, it need hardly be said, yielded the merest trifle, but he was fortunate in holding examinerships in the University of London, Cambridge, and the Society of Apothecaries; he was also for several years botanical referee to the Royal Horticultural Society. In 1863 he received one of the Royal Medals of the Royal Society for his researches on the reproductive organs in Thallogens, &c.; and in 1879 a Civil Service Pension of £100 per annum was awarded him for his investigations on the diseases of agricultural crops, &c. He was elected a Fellow of the Linnean Society in 1836, and of this Society in 1879. Shortly before his death, which took place at Sibbertoft, on July 30th, 1889, he was made an Honorary Fellow of his Cambridge College (Christ's). He married in 1833 Miss Cecilia Emma Campbell, by whom he had, as stated above, fifteen children, ten of whom survive him.

J. D. H.

Sir Robert John Kane, LL.D., F.R.S., who died in Dublin on the 16th of February, 1890, in his eighty-first year, belonged to the distinguished group of chemists whose chief scientific work was accomplished during the first half of the present century. Sir R. Kane's contributions to chemical science almost ceased after 1850, when his official relations with the Irish Government drew off his attention to the economic and educational problems of the time. His scientific knowledge, scholarly attainments, and experience were then freely utilised by the State in the efforts made to establish the system of education which was, until very recently, represented by the Queen's University and its Colleges at Cork, Belfast, and Galway.

Born in 1809 at Dublin, where his father had established a chemical factory, young Kane was educated in his native city, and ultimately (in 1835) graduated in Arts at the University, whose LL.D. degree was subsequently conferred upon him (*Stip. condonatis*). The family connexion with industrial chemistry seems to have early attracted him to scientific pursuits, and the first results of his chemical work appeared in 1829, in the form of two short papers on Native Compounds of Manganese, including the description of an arsenide of manganese, since known as "Kaneite." But before the publication of these papers Kane had commenced the study of medicine, apparently with a view to adopt it as a profession, for he became clinical clerk to Graves and Stokes at the then celebrated Meath Hospital, in Dublin, and went to Paris in 1830 to continue his medi-

cal work. Here, however, he attended Chevreul's lectures, and became acquainted with the brilliant Dumas, whose enthusiasm seems to have revived the young Irishman's love for chemistry, and decided his choice of a scientific rather than a medical career.

Shortly after returning to Dublin, in 1831, Kane obtained the Lecturership in Chemistry at the medical school then maintained by the Apothecaries' Company of Ireland, and this office he held until 1843. Most of his time was now devoted to scientific teaching and investigation, while a portion was occupied in completing his studies in Medicine and Arts. Although he never practised as a physician, he obtained the licence of the Dublin College of Physicians in 1835, and was elected to the Fellowship of that body in 1843.

When fairly established in professorial work Kane commenced the examination of some compounds of the metal platinum, and published accounts of the stannous chloroplatinite, of a substance which he regarded as platinoso-platinic iodide, Pt_2I_6, and of other bodies, in the 'Dublin Journal of Medical and Chemical Science.' But his work on the salts of some of the complex platinum bases derived from ammonia only appeared at the end of a paper in the 'Philosophical Transactions' for 1842, entitled "Contributions to the Chemical History of Palladium and Platinum." This paper was chiefly concerned with the compounds of palladium, of which he described a suboxide and a corresponding chloride, Pd_2Cl_2; while the action of alkalies on palladious chloride, $PdCl_2$, afforded several basic substances which he regarded as definite compounds. In the course of the same investigation Kane produced a number of interesting bodies by the action of ammonia on the salts of palladium, which doubtless included derivatives of the bases *palladamine* and *palladiamine*, subsequently recognised by Dr. Hugo Müller in his fine investigation of similar interactions.

During the "thirties" much progress was made in evolving order out of the apparently chaotic masses of organic compounds. Early in the decade Dumas had propounded his ephemeral "etherine" theory of ordinary alcohol and its derivatives; Liebig and Wöhler had been led to recognise the existence of compound radicals; and, later on, the laws of substitution were made out by Dumas, and the theory of types was proposed. Kane's acquaintance with Dumas and association with Liebig—in whose laboratory he sometimes worked during the summer months—led him to take an active part in the discussions of the time, and to propose the theory of the nature of common ether and alcohol which now prevails—namely, that they include the radical ethyl, C_2H_5. It is true that Berzelius arrived at the same conclusion about the same time, and worked out the subject with his usual thoroughness; but Kane claimed to be the independent discoverer of what was then termed the "Ethyl Theory."

The inquiries being pursued, at the period of which we write, with regard to compounds of the alcoholic class, led Kane to re-examine pyroxylic, or wood, spirit, which had already been investigated by Dumas and by Liebig, though these chemists had obtained somewhat discordant results. Liebig's products were shown by Kane to be impure, as the alcohol used seemed to contain some methylal, ethylene dimethylate, and other bodies. Kane then succeeded in arranging the process for separating nearly pure methyl alcohol from wood spirit with which his name is now generally connected. The success of his method depends on a fact which he had observed, namely, that methyl alcohol forms a definite and crystallisable compound with calcium chloride which is not decomposed at 100° C. in absence of water; hence dehydrated wood spirit, when saturated with anhydrous calcium chloride, can have nearly all its volatile impurities distilled off, leaving the calcium chloride compound with methyl alcohol; the latter, if then mixed with water, is decomposed, so that a second distillation affords nearly pure methyl alcohol which only needs dehydration. From the alcohol so obtained Kane prepared and described several salts of methyl-sulphuric acid.

Among the volatile compounds mentioned above as bye-products from the purification of methyl alcohol was the liquid now termed *acetone*, but then, generally, "pyroacetic spirit." At the time (1836–7) acetone was known to consist of C_3H_6O,* but the nature of the compound was undetermined; and this problem Kane sought to solve. The results of his investigation led him to conclude that acetone is a hydrate analogous to ethyl alcohol, but containing a radical which he named "mesityl" $= C_3H_5$. A number of derivatives were prepared by Kane from the impure "pyroacetic spirit" he worked with, which seemed to support his view of the nature of acetone. Thus he obtained an oxide which he named "mesityl oxide,"† because it was apparently related in composition to acetone as ethyl oxide is to ethyl alcohol; again, by dehydrating acetone a hydrocarbide resulted whose empirical formula proved to be C_3H_4;‡ this he named "mesitylene," as in composition and mode of generation it seemed to arise from mesityl alcohol (acetone) as ethylene, C_2H_4, does from ethyl alcohol. Later on, as new facts were discovered about acetone, it became evident that the alcoholic hypothesis was not consistent with them, for the compound was found to possess aldehydic rather than alcoholic characters. According to the newer view acetone was acetyl

* $C_6H_6O_2$ according to the atomic weights for carbon and oxygen then used.

† Later on Kane discovered a liquid product of the action of heat on acetone, which he named "Dumasine." This has since been proved by Fittig to be isomeric with mesityl oxide.

‡ Hofmann has shown that this important body, which Kane discovered, has the molecular formula C_9H_{12} ($=3C_3H_4$), and is symmetrical trimethylbenzene.

methide, ordinary aldehyde being acetyl hydride; this received powerful support from Williamson's study of the genesis of acetone and its homologues by heating barium salts of the fatty acids; while Freund's synthesis of acetone by means of zinc ethide and acetyl chloride, together with the work of Boutlerow, Friedel, and Crafts, involved a further modification, so that acetone and its homologues are now simply regarded as compounds of carbonyl with two alcohol radicals. Thus the difficult problem undertaken by Kane more than half a century ago, when very vague notions prevailed as to the constitution of some of our most common carbon compounds, required the more powerful methods of modern chemistry for its complete solution.

The derivatives of ammonia early attracted Professor Kane's attention, as his friend Dumas had shown in 1830 that oxamide, $C_2O_2(NH_2)_2$, included the ammonia residue NH_2, which he termed *amide*; and that many substances existed which might be supposed to contain the same group. Kane, starting with this general idea, commenced the examination of a number of inorganic compounds which might contain the group NH_2. The most interesting of these is the "white precipitate of mercury," which had long been known, and about whose composition conflicting statements were made. Kane studied the formation of that body with great care, and showed that a definite compound free from oxygen could be obtained under proper conditions, whose composition is represented by the formula $HgNH_2Cl$. He was led by his work on this substance, and on similar compounds of other metals, to conclude that the three atoms of hydrogen in ammonia, NH_3, are not removed with equal readiness in chemical exchange, and that ammonia is, in fact, the hydride of a persistent group, NH_2, which he termed *amidogene*.

Kane's amidogene theory, and its consequences as affecting the view taken of the constitution of ammoniacal salts, attracted great interest at the time. That Berzelius attached much importance to Kane's work is evident from the remark attributed to him in Wöhler's 'Jahresbericht' for 1838, "Diese Untersuchungen von Kane gehören meiner Ansicht nach zu den wichtigeren des verflossenen Jahres." Although subsequent investigations of Wurtz, Hofmann, and many others have shown that the three atoms of hydrogen in ammonia can be successively replaced by methyl or ethyl without affording the metameric mono- or di-substituted derivatives which might be expected to exist on the amidogene theory, it is interesting to note that Curtius, working nearly fifty years after Kane's papers were published, has succeeded in obtaining the hydrate of diamidogene, $NH_2—NH_2$, or as it is now termed *hydrazine*.

Kane was awarded the Cunningham Medal of the Royal Irish Academy in 1843 for his researches on the above subject.

In 1841 Kane received the Royal Medal of this Society for his work published in the 'Philosophical Transactions' for 1840 on the chemical history of the well-known substances archil and litmus, which are colouring matters obtained from various lichens of the *Rocella*, *Variolaria*, and other genera. When the lichens are allowed to ferment in presence of ammoniacal salts, they soon afford the magnificent purple-red colouring matter termed *orceïne*. But if carbonates of the alkalies are present during the process of fermentation, blue litmus results. The two colouring matters were previously examined by Robiquet, Heeren, and Dumas, yet Kane carried the investigation much further, and not only improved the methods of separating some of the substances present in the impure pigments, but endeavoured to trace them to proximate constituents of certain of the lichens. In the course of this laborious investigation he obtained a number of new substances which he regarded as definite compounds. The general result of the inquiry was that the red "orceïne" of archil, and the blue substance of litmus, named by Kane "azolitmin," differ only by one atom of oxygen, the blue coloured body containing most oxygen; and that both colouring matters are products of the action of ammonia and atmospheric air, in presence of a ferment, on orcin already free in the lichens or resulting from the hydrolysis of some of their constituents.

In 1842 Kane published an account of his examination of the colouring matter of the berries of *Rhamnus tinctoria*, one of the buckthorns, which were imported in considerable quantities from the Levant for dyeing, under the name of "Persian berries." From these when in the unripe state he isolated a golden-yellow colouring matter, which he named *chrysorhamnine*, and from the ripe berries, olive-yellow *xanthorhamnine*, and showed that the latter results from the action of oxygen and the elements of water on chrysorhamnine.

Plant products always seemed to have a great attraction for Kane, and amongst others the volatile oils, with which he worked much from time to time, though he published little about them, as he found greater difficulty than he anticipated in arriving at a "law connecting the composition of the secretions of plants of the same genus or natural family." But he made some useful contributions to our knowledge of these oils, as well as to that of other minor subjects, which the limits of this notice do not permit us to specify in detail.

In 1843 Kane was appointed Professor of Natural Philosophy to the Royal Dublin Society, and during the following year published his 'Elements of Chemistry,' a work which well represented the general theories and the practice of the science at the time. But his connexion with the Royal Dublin Society—a body which has always been foremost in seeking to develop agriculture and industry in

Ireland—gave a new direction to his energies. He commenced a protracted investigation of the "relations of the country to the prime materials of the chemical and metallic manufactures," and was led much beyond the original limits of his inquiry "to discuss several important statistical and moral problems affecting the industrial progress of Ireland." The first part of the investigation involved a large amount of analytical work on native ores and raw materials, and the results were embodied in a course of lectures delivered before the Royal Dublin Society. In these lectures it was also pointed out that foreign Governments fostered native industries with anxious care, and that Continental nations were in consequence making giant strides in manufacturing activity, while our own Government did little or nothing to develop the resources of the country. These lectures were published in 1844, and excited such interest that 'The Industrial Resources of Ireland' quickly reached a second edition, and the Government of the day shortly after made one step forward in the direction indicated by Kane in establishing a "Museum of Irish Industry," at St. Stephen's Green, of which he was made the first Director. In this office he devoted much time to the development of the museum under his control, and especially to the establishment of evening lectures, and practical instruction for artizans and others who could not attend day classes. This excellent technical scientific school proved a most important adjunct to the museum, and supplied a definite want; it continued its valuable, if humble, work until converted into the "Royal College of Science for Ireland."

The honour of knighthood was conferred on the subject of this memoir in 1846, and in 1849 Sir Robert Kane was elected to the Fellowship of the Royal Society. In the latter year Sir Robert's official connexion with the Museum of Irish Industry almost ceased, as he went to Cork in the capacity of President of the Queen's College, then recently established in that city. The duties of that important office he zealously performed until 1873, when he resigned, and returned to Dublin, where he afterwards lived in comparative retirement.

Although Sir Robert Kane's official engagements prevented the pursuit of his old work, he continued his connexion with the 'Philosophical Magazine,' one of whose Editors he was from 1840. After his return to Dublin, in 1873, Sir Robert became a Commissioner of National Education; he also took much interest in the work of the various societies of the Irish metropolis. He was for some years President of the Royal Geological Society of Ireland, and from 1877 to 1882 of the Royal Irish Academy; he occupied a seat on the Academic Council of Dublin University, and on the Senate of the Royal University. In the various public positions which Sir Robert Kane filled during a long and distinguished career, his natural

courtesy and diplomatic skill enabled him to disarm much opposition to reforms which his mature judgment and statesmanlike grasp of affairs led him to suggest.

Notwithstanding advanced years, Sir Robert Kane's health was generally good, but the end came after a very short illness, and one morning last February many gathered at his funeral to honour the memory of one whose scientific reputation belonged to a past generation, and whose later life was largely devoted to the advancement of his country's material interests.

J. E. R.

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